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PERSPECTIVES ON GOVERNMENT AND SCIENCE

Special Editor of this Volume

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FOREWORD

An issue of The Annals devoted to government and science needs no justification. The challenge to our scientific and technological position, implicit in Soviet space conquests, has resulted in a virtual consensus that government should act to correct such deficiencies as may exist.

But this consensus disappears amid the welter of diagnoses as to what is wrong with our system and an even greater quantity of prescriptions and panaceas as to what should be done.

The national government is clearly committed to a dominant role in stimulating, promoting, and supporting science, scientific research, and technological development. Since World War II numerous programs have been initiated and tremendous sums have been made available for research and development. And the willingness of Congress to underwrite further expansion is not flagging. Cold war threats and general competition with post-Sputnik Russia effectively deter tendencies toward niggardliness or economy in this sector of public expenditure.

Yet the very ease with which financial support for scientific programs can be secured may obscure many difficult and perplexing policy and administrative problems connected with the role of government in scientific research and development. Money alone will not eliminate shortages of trained manpower. It may even complicate scientific progress by, among other things, intensifying bureaucratic tendencies and increasing restrictions on desirable freedom for research. Money may also distort the content of research programs and divert the application of research energies. And in any case, financing is but one part of a complex problem.

In a recent address Dr. George B. Kistiakowsky, the President's scientific adviser, suggested the fundamental character of the challenge facing the American people, by emphasizing that the country needs a general upgrading of its citizens' intellects, if we are to progress scientifically. He pointed out that even the generally held concept of science is usually erroneous in that the average citizen gets technology all mixed up with science and confuses products that enhance human comforts with ideas of science.

"Our national objective," Dr. Kistiakowsky declared, "should not be the creation of an elite corps of intellectuals, but rather one of raising and improving the intellectual tone of our whole society. . . . We cannot achieve a desirable level of scientific activity until there is not only its acceptance by society but a conscious desire to cultivate and encourage intellectual excellence."

If indeed Dr. Kistiakowsky's description of the problem is somewhere near the truth—and I fear that there is much evidence to support his views—then the problems facing us are truly tremendous. They will not be solved by mere tinkering; they will require a reshaping of public attitudes, a redirection of public energies, for we are then talking about social, political, and economic change.

This issue of The Annals presents no consensus of views, but it does present some clear and appropriate analyses. Selection of the topics for treatment was no easy task. Some subjects which are obviously important are not dealt with. Articles on rocketry and space research, on science and foreign policy, on scientific manpower, on geophysics, and on many other topics would have been appropriate and significant additions to those which are included. But space limitations prevented their inclusion. Nevertheless, an impressive start has been made by the several authors in exploring the subject of government and science and in laying a foundation for an understanding of some of the complex policy and administrative problems in this critical field of national interest. Unquestionably, it is only through such exercises in critical analysis that we can move in the direction of creating the values which Dr. Kistiakowsky feels are so necessary. If this issue contributes in a small way to such a re-examination of basic values, it will have justified itself fully.

I would, incidentally, like to express my appreciation to Mr. Edward Dawson, who assisted with the preparation of this issue during the four months that I was out of the country.

NORMAN WENGERT

Norman Wengert, LL.B., Ph.D., College Park, Maryland, is a Professor of Public Administration at the University of Maryland. He is presently on leave, serving as Deputy Director for Studies of the Outdoor Recreation Resources Review Commission. He has been a Research Associate with Resources for the Future, Inc., and has served as a staff member in the Office of the Secretary of the Interior. He has also held various administrative positions with the Tennessee Valley Authority. He is author of Natural Resources and the Political Struggle (1955) and Valley of Tomorrow: TVA and Agriculture (1952).

The Challenge of Government Science

By John C. Honey

Abstract: The failure of government science to meet Russian accomplishments in space research poses fundamental questions about American governmental and social institutions which are of deep concern to public administrators. These questions do not find their solution in relatively minor adjustments of the administrative machinery such as have been made since the fall of 1957. They relate to our pluralistic values, our lack of a sense of national purpose and our system of Presidential leadership made weak by the checks and balances which are grounded in the Constitution.

The contrast between Russian and American scientific capacities, and the relative ability of the two societies to plan and to direct resources to those activities which further the national interest, lead to the following basic questions: do we need constitutional reform to provide for a stronger Presidency and for a modification of the costly and inefficient federal system? Must we not anticipate national planning so as to assure that the creative and productive capacities of the country go first to efforts of high national importance? Beyond these basic questions there are a number of urgent matters which challenge public administrators, for example, the quick development of an effective research communications system and the treatment of scientists in government in a manner commensurate with their importance to the national well-being.

John C. Honey, Ph.D., New York City, is Executive Associate of the Carnegie Corporation. He previously served four years as director of government studies in the National Science Foundation. Prior to that time he was on the program planning staff of the Secretary of the Interior. A graduate of Bard College and The Maxwell School, Syracuse University, he has also taught at a number of institutions and has conducted research for the Office of Naval Research on problems of large-scale research organization.

IT COMES as a shock to realize that the challenge of government science to public administration today lies largely in the failures of governmental science. The challenge, of course, reaches far beyond public administration, even when broadly conceived as embracing every aspect of the governmental processes. American society as a whole is involved in this failure of governmental science.

Although federally financed research and development amounts to something less than half of all science in this country it is, from the public point of view, the most responsive and responsible. Industrial firms pursue research as an adjunct to their individual productive efforts and economic objectives. Universities encourage it to advance knowledge in general and to enlarge the capabilities of their faculties. But government conducts and supports science in the fulfillment of its national responsibility. When the national interest suffers a severe setback through the inabilities and failures of major government research programs, as it did in the fall of 1957, and has on numerous occasions since, it is unavoidable to conclude that the processes of government—public administration—face a serious challenge indeed.

How could failure arise out of the great structure of American governmental science with its thousands of dedicated and creative servants, its hundreds of laboratories—some governmental, some located elsewhere but financed with public funds—its unending flow of marvelous achievements? And how on the other hand was it possible for the Soviet Union, which only forty years earlier had been in the throes of revolution, which less than two decades before had been physically wracked by the greatest war in history, to show scientific progress well beyond American capacity?

These are questions which thoughtful citizens, including many directly engaged in the governmental processes, have been driven to ask. In spite of early assurances from some politicians that the gap in accomplishment between Soviet and American science was nonexistent or of little moment, the continuing evidence belies such views. The notion that we had never intended to be in the race with Soviet science in the exploration of space reassured only the most naive. Governments are responsible and effective only insofar as they succeed in meeting national needs and interests. It could scarcely be argued, for example, that the impetus given by Russian accomplishments and American failure to the nations of Asia and Africa to find their solutions in the Communist pattern, were of no significance to the national interest.

Weaknesses in government science

Observers of the government scene had been concerned prior to 1957 with a number of weaknesses in the relationship of government to science. The organization for the conduct of scientific affairs was clearly chaotic. At the top there were several bodies, notably the National Science Foundation, the Interdepartmental Committee for Research and Development, and the President's Science Advisory Committee, in addition to the National Security Council, all with responsibilities for advising the President on national science policy. The respective roles of these organizations, as policy framing and advising agents on scientific matters, were at many points unclear. Furthermore, several of the most important research bureaus were illogically and inhospitably housed in major departments. Others existed in a kind of "power limbo" as independent agencies.

Leadership in this period was a matter of special concern to those who saw the government's research programs as particularly crucial. The interest in scientific affairs in the White House was tepid; the advisers on science policies were timid or distracted by other responsibilities, or unsure of their authority. The derogation of basic research by a Secretary of Defense in this period and the vacillation over defense research budgets were symptomatic of executive branch attitudes. It was not possible, either, to detect extraordinary Congressional leadership.

As for the scientist in government, he had been assaulted by McCarthyism and had witnessed the spectacle of a Secretary of Commerce siding with parochial business interests against the detached findings of the National Bureau of Standards in the famous battery additive case. Furthermore, the government scientist was paid as other civil servants; no better in spite of the unique function he fulfills. He could rarely attend professional meetings; could almost never travel abroad. His communications and publications were restricted by security regulations, many of which transcended the bounds of sense and utility.

The government's current and anticipated needs for scientists and engineers were largely unknown. This was equally true for the universities and industry. In consequence it was not possible to give informed attention to the potential supply in the universities of the country and to provide guidance and incentives for spurring the output of technically equipped manpower. Education proceeded as it always had, a highly individualistic operation.

Science in the Soviet Union

Since October 1957, there has been a gradual realization of the true nature of the status of science and education in Russia. We have learned of the national dedication to learning which pervades the Soviet Union. We are beginning to understand the extent to which the communist leadership is willing to divert resources to education and science. The able, technically trained person occupies an honorific position quite different from that which is his lot in the United States. His material rewards are high, so high in fact that it would be a severe downward adjustment for him to exchange positions with American university and government scientists. He is aided in his work by excellent equipment; he has access to practically all important foreign scientific works, in translation. Perhaps of greatest significance is the evidence that Rusian science is largely carried on in a context of social purpose and that creative talents still produce with remarkable vigor.

New American response

The last two years have witnessed two levels of response with respect to American science and the research programs of the government The first of these is action-directed, and is evidenced by many administrative steps described in other articles of this issue. The second level of response is found in a greater sense of urgency and a growing feeling that more profound problems and more fundamental changes may be implied in government science than have yet been widely articulated.

This deeper, second level of response probably reflects an expanding awareness among American citizens that Russia's achievements in science threaten the United States and all democratic governments, not only militarily but in the very essence of their institutional arrangements for implementing the philosophy of popular sovereignty. Some of the problems related to national purpose, leadership, goal-setting and planning which come to the fore as we look at the current state of governmental science are discussed in the following pages.

NATIONAL PURPOSE AND LEADERSHIP

With growing frequency these days one hears in responsible quarters that the United States lacks a sense of national direction. Contrasts are drawn to the Soviet Union where American visitors are finding a widespread urgency and purpose in every type of activity. The Soviet problem is simple, of course. National purpose has been predetermined by the leadership. It is unitary, easily articulated, and constantly pressed upon the citizen. In essence it is the development of communist society and its extension to other parts of the world. What is meant by "communist society" is growing in attractiveness: in terms of security, increasing material comfort, excellent educational opportunities, health benefits, recreational and artistic outlets. Of great importance is the sense of national *esprit* associated with accomplishments in space research and exploration.

In the United States the noble ideal of individual self-fulfillment has been and remains the central theme of the nation. This objective, worked out in a frontier environment and under the aegis of a governmental structure designed to dilute authority, has resulted in an astonishing pluralism. Every man sets his own goals, and in ordinary times his conscience as much as his government dictates the degree to which he will actively contribute to the national interest. The multitude of goals and the high degree of similarity in range of interests of the two national political parties reflect the pluralistic nature of our society. It also suggests, as does the history of third parties in this country, that unitary approaches seem to be antithetical to our national make-up.

The problem which presents itself in the competition for national progress between Russia and the United States is well illustrated in the treatment of scientists and engineers and in the provisions for technical education in the two countries. The success of communism depended in great measure on the expansion of the technical base of Soviet society. In consequence attractive conditions have been provided for scientists and engineers, and scientific accomplishments are widely rewarded and honored. Technical education and scientific education have been made the birthright of the able and gifted and are provided without cost, in fact with subsidization of the individual. In the United States, while we admire the creative scientist, we have done relatively little to reward his unique capacities and contributions, in part, on the grounds that egalitarianism forecloses undue attention to the gifted. Few special educational opportunities have been available to the potential scientist. Talent has risen to the top through the interplay of many social and individual forces and it has been smothered and lost in a similar manner. The planned emergence of the gifted to posts commensurate with their capacities has been unheard of in the United States.

Our national purpose

The only occasions upon which it has been found fitting to enunciate a national purpose have been emergency ones—in this century the two great wars and the great depression. And in the latter case national purpose rapidly dissolved into partisan feuding as soon as the first remedial steps to avert economic collapse had been taken. In these periods science found its place in the scheme of the national effort;

through the National Research Council in World War I; through the National Resources Board's report, "Science—A National Resource"; and through the Office of Scientific Research and Development (OSRD) of World War II.

It grows clearer with each emerging piece of evidence on Soviet Russia and Red China that the purposes of the state are exercising a powerful influence on the peoples of those nations. Lacking the physical threat of depression or war our own national purposes remain as diffuse as during any peacetime period in the past.

The fundamental question is thus posed whether the United States, given the highly individualistic character of the goals which move its citizenry, can meet the competitive challenge of communism. In the abstract and ideal situation, each citizen would be animated so that his energies would be distributed in appropriate amounts to meeting social responsibilities and to fulfilling his own capacities. The practical problem for the student of government is to determine whether measures may be taken which would provide clearer opportunities for the enunciation and implementation of national purpose.

Limitations of our constitutional system

This being a time of national and cultural peril unparalleled in American history, it is perhaps not too imaginative to speculate about some elements of our constitutional system and their utility for the present. We seldom reflect on the possibility that the act of genius of 1789, if blindly adhered to in the twentieth century, may spell national failure. We infrequently ponder on whether the rate of adaptation to modernity which our system of judicial review permits is so slow that as a nation we may already be on the brink of displacement as a great power. The economic and political values which motivated the framers of the Constitution were eighteenth-century responses to mercantilism, autocratic government, and the needs of a loosely knit agrarian community. The tightly interwoven, heavily populated, complex society of today may not be able to function effectively upon terms set so long ago.

Do we find, for example, a frustration of the means for framing and enunciating national purpose in the built-in competition between Presidency and Congress? History, and administrative experience, demonstrate beyond doubt that strong institutions have strong spokesmen. Under our prevailing system of checks and balances, Presidential leadership of a high order comes about only through the juxtaposition of crisis situations and forceful personalities. We have no assured continuity of leadership in the office of the Presidency and hence one of our primary sources for the generation and articulation of national purpose is in constant ambiguity. The Office of the Presidency, freed from some of the constraints of Congressional review, less limited in ultimate program and fiscal responsibility, and more able to move at home and abroad—that is, unhampered by Congressional and judicial pressures—might be a source of national purpose which is now seriously wanting.

Important issues

Under such circumstances there could be a resolution of one of the important issues affecting science that has confounded the federal government for many years, namely, the level of support to be devoted to fundamental research. There is no absolute answer to the problem; whether we encourage basic research depends on many factors. For example, does the scientific community find the flow of basic knowledge adequate or is much applied and developmental work frustrated because we lack underlying facts? Are other nations supplying us with basic knowledge and is this a reasonable arrangement given our own strong inclinations to applied and practical work? Are the talents of the scientific community for fundamental research being adequately utilized or are they being diverted to the production of hardware? These considerations and many others go into the ultimate formulation of a position on fundamental research. If the national interest suggests, after reasonable exploration, that increased funds must be diverted to basic research, the strong Presidency could effect such a move. As it is, the currents that flow around the processes of making funds available for fundamental research are so numerous and unpredictable that real doubt remains as to how well the national interest has been served.

While we tend to argue that what emerges from the prevailing system of give and take serves, in general, the common good, this supposition too must be questioned. It is conceivable in an age of rapid change and ever rising challenge that the pressures exerted through this system reflect a parochialism which has little bearing on the true national interest.

Similar doubts must be raised about the efficacy of the constitutional distribution of responsibilities between states and central government. The trend has long been toward the modification of the states from sovereign powers within the federal framework to administrative units. Can we risk awaiting the outcome of the evolutionary process or are more radical measures implied, such as possible constitutional reform?

These are profound and serious questions, ones which students of government and administration may need to explore in earnest as the challenge of communist accomplishment grows.

POLICY AND PROGRAM FORMULATION

In the absence of clear national goals which the nation is striving to fulfill it is not possible to develop in our research and development programs the sharp sense of direction and purpose which will permit a determination of priorities and emphases. The technically trained man in automotive engineering is, under the prevailing scheme of things, performing work no different in value from that done by the engineer engaged in government rocket research. Indeed, if wages be the criterion we place a considerably higher value on automotive engineering.

There are profound implications for American society in the realization that the communist competition is demanding that we set priorities and direct application of our inevitably limited resources to those activities which most directly affect and serve the national interest. One can foresee the modification of our entire system of production and distribution, with controlled output of consumer goods and a severe restriction on the peculiarly American institution of "want-creation" as a likely development. Public service oriented activities would expand faster than at present since the government would divert a higher proportion of the national income to education, research, health, housing and cultural endeavors.

If such predictions are accurate the national government will need to devise and administer systems to control production and distribution. It will have to find means of diverting resources to socially desirable ends while retaining such essential ingredients of democratic society as widespread consultation and channels for the expression of dissent.

A tremendous challenge will be presented to public administration under such circumstances.

Planning of research and development

Exploration of these larger social, economic, and cultural implications of the present crisis goes beyond the intended scope of this paper. However, we can appropriately look at the existing arrangements for program planning especially with reference to how they affect the government's research and development activities. For in this area it is possible that some practical moves for improved practice can be made with relative ease. Thoughtful attention to the program-planning process inevitably forces a concern for clear articulation of national objectives, knowledge about the current status of the nation's strategic position, and knowledge about manpower and resource supplies.

At present, planning for the government's research and development activities is no more well developed or long range than is planning for any governmental functions. Since nearly all such research is carried on for practical purposes it is bound, in its conception, to reflect the approaches taken in the government to planning and programing generally. Characteristically, federal operations are conceived on a short-term basis, reflecting the budget and appropriations systems. Lacking long-term goals, government thinking is usually dominated by a "crash" and *ad hoc* atmosphere. The research and operating problems being faced reflect the questions which have previously been posed and these in turn depend in great measure on the machinery employed.

Planning under the National Security Council

At present, the broadest issues related to the national security and defense are handled in the National Security Council (NSC), which assesses the nation's security posture at any given time in the light of the evidence available, and arrives at judgments which are then recommended to the President. The military research and development program is designed to be consonant with the policy positions which emerge from NSC deliberations and subsequent Presidential decisions. Crucial to the matter of how foresighted the NSC can be—how anticipatory of the major future lines of action which the government must pursue—is the nature of its planning. Authoritative critics say that since it is composed of several Department representatives it suffers on two counts: first, the immediacy of operating responsibilities colors its sights and forces it to concentrate on the here and now rather than the future; and second, it can never serve as a detached, reflective agent to examine future needs and responsibilities of the government since it must try to resolve conflicting departmental views and come up with compromise solutions for the President.

Given such national security planning machinery it is not surprising that research and development planning has been too little oriented to the more distant future but rather too much concerned with immediate problems and conflicts. While there are many contributing factors, certainly the foregoing explains in part our failure with respect to space research. Farsighted planning would have included a vigorous program of outer space experimentation, recognizing the extreme significance to the free world of maintaining a posture of pre-eminence in this technical and scientific realm.

In another respect the National Security Council arrangement is highly unsatisfactory for planning purposes; one which bears directly on research and development. While the NSC has been concerned with the national supply of technical manpower, it has never been in a position to see this problem in the context of the entire problem of education in a democratic society. This is precisely because of the security orientation in the NSC mandate. Until the training of scientists and engineers can be dealt with as one facet of the national problem of future manpower needs, until their training can be planned for a substantial period ahead, say at least ten or fifteen years, taking into account industrial, university, governmental, and other requirements, we are bound to come up with fragmentary and distorted results.

Other federal agencies

In recent months the Office of the Science Adviser to the President has expressed the view that we have need for long-range planning to determine the direction in which the national scientific effort should move. Indeed the intention has been expressed by that office to engage in such planning. However, it seems certain that the planning of research and development, including attention to the problems of education and to the encouragement of basic research, needs an environment of greater generality and more detachment from operational demands than can be found in either the Office of the Science Adviser or the NSC.

The other agency at the Presidential level which is, in fact, concerned with all activities of the executive branch is the Bureau of the Budget. Its functions of policy and fiscal review and the limitations inherent in this review process are almost too well known to mention. The Bureau has been, especially in recent years, the President's agent for effecting economy and for striving to bring expenditures and income into balance. But at no time in its history has the Bureau been sufficiently detached from the annual budget process to permit the kind of perspective on departmental and agency plans and programs, to allow it to qualify as a planning body.

In the departments and agencies planning is still in a most rudimentary state. A few departments have had, or now possess modest units concerned with policy and program development which use secretarial services in common. In some instances research has been considered beyond the scope of the planning units. By and large, in spite of the heavily applied character of departmental research, there has been little interplay at a planning level between policy, operating and research people.

A POSITIVE PROGRAM

One can readily construct a formula for the rational determination of goals and program plans to embrace the totality of government responsibility including research and development. This formula might consist of a Presidential planning board or Chief of Planning in the White House, equipped with staff and authority to muster the extensive information required to formulate long-term, five- or ten-year, goals and schedules of annual programs With such a central planning body in the White House, complementary planning units might be found in each department and agency, and in the major subunits thereof. These would have access to a common pool of basic data about the national condition, for example, population projections, economic indicators, available and anticipated resources, and manpower supplies.

Formulas of this kind are easy to construct in the abstract, and are perhaps far from the reality of any national planning machinery of the future. But they serve to suggest the nature of the need, and speculation may be warranted.

since few public administrators, be they students, scholars or practitioners, if the literature is indicative, are directing much thought to the development of such machinery. The public-planning function has been given lip service for a generation as an element in the administrative process. But the planning which receives most attention is directed to urban government and to physical planning, in a quite limited sense.

Numerous other opportunities for administrative inventiveness are at hand in the government's research endeavors. We need to develop really adequate arrangements for communicating research findings and to provide rewards and incentives to the scientist in the public service which are commensurate with his value and not simply with egalitarian philosophy. We need to channel funds to the secondary schools and universities for classroom and laboratory construction purposes, as well as to assure scholarships for all persons of sufficient ability to profit from higher education. We need to discover better means of expeditiously concentrating large-scale resources on major research problems, where the national interest is involved, and where the heavy cost of research tools precludes ordinary approaches. It would also be desirable to work out with the states allocations of mutual research responsibility in shared program areas.

However, attention on the part of public administrators to such specifics as these should not convey an impression that the government is now successfully meeting the competitive challenge from Soviet Russia.

Only when we see a widespread public interest in facing the fundamental problems inherent in our own political and social structures can we feel hopeful that popular government may be able to meet the challenge of communist accomplishment.

Recent Developments in the Relationship of Government to Science

By E. R. Piore and R. N. Kreidler

Abstract: National concern for science has increased as the recent developments in the relationship of government to science testify. The government's extensive role in scientific activities is witnessed by the numerous agencies and department divisions which have been created or extended in their scope. The growth of government-science operations has brought with it the present experimentation for a better administrative structure. One of the newer attempts to promote co-ordination in governmental science was the Presidential appointment of a Special Assistant to act as a liaison between government and science in the executive branch. He is particularly well suited for this role of co-ordinator as chairman of both the Science Advisory Committee and the Federal Council for Science and Technology. The augmentation of the government's role in science and the accompanying proliferation of scientific agencies has created a need for scientists who are both versed in technical subjects as well as in the operations of the government. These specialists may be secured if the academic institutions realize the nation's growing need.—Ed.

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DURING World War II and in the period following, members of the scientific community have evolved relationships to government and national policy quite similar to those developed by economists, lawyers, and social workers during New Deal times in the nineteen thirties. Beginning with Vannevar Bush, a succession of men, including Oliver T. Buckley, L. A. Du Bridge, I. I. Rabi, Detlev W. Bronk, James R. Killian, Jr., and, at present, George B. Kistiakowsky, have served both as chief delegate of the scientific community and as symbol of the involvement of science in national policy.

This involvement, which started with the restricted technological requirements of a "hot war," has grown with the mounting diplomatic and economic pressures of the "cold war" to include a far-reaching concern with problems of national survival and world leadership. Indeed, its present scope may be described as "intellectual preparedness" for the requirements of national and international life in today's revolutionary world.

The first outlines of this broadening involvement of science with government are recorded in the Senate hearings of the Seventy-ninth Congress. At that time the Senate was exploring the problems of transition from war to peace and assessing and evaluating alternatives for the postwar period. Out of these hearings, especially the testimony of Vannevar Bush, came the first of a series of new government agencies which were to codify and give substance to the newly important and newly identified relationships between science and national life, and between scientists and public policy.

This article will seek to explore some of these relationships and the need for public understanding of the reasons for their existence. It also will be concerned with what can be expected of them, and how these new relationships are developing into an institutional framework, the normal device by which citizens manage their public affairs in a democracy.

THE NEW AND THE OLD

Before beginning this discussion, a large part of which will be concerned with the creation of new government agencies dealing with science, two observations need to be put on the record.

First, the Atomic Energy Commission, the Office of Naval Research, and the National Science Foundation, the major innovations in the family of new federal agencies for science, were truly a new approach in a major sense. Yet, they also were logical outgrowths of wartime agencies, such as the Office of Scientific Research and Development and the National Defense Research Committee. Moreover, they had historical roots in a whole family of institutions going back to the early years of the Republic. The Public Health Service, the Department of Agriculture, the Geological Survey, the Weather Bureau, the Coast and Geodetic Survey, the National Bureau of Standards, and the National Research Council of the National Academy of Sciences are only a partial catalogue of the long involvement of government with science.

While recognizing that something new between science and public policy began to happen in the postwar years, it also is clear that it was not so new as to be without roots and precedent in our national past. To emphasize this continuity is not only useful to those with a taste for the threads and patterns of history, but also is of some significance in understanding the evolution of the broadening relationships of government to science which are still in midstream.

Parallel with the 1930's

The second observation takes us back for a moment to the parallel with the New Deal when a new family of agencies was created to carry out public policy in the economic and social areas. At that time, many new public functions which might have been lodged within existing cabinet departments were vested, instead, in newly created agencies outside of departments. This proliferation of independent agencies was motivated, in part, by the pragmatic urge to get on with the job, but also by the desire in Congress and the Executive Office to observe the efficacy of these operations in some detail. Independent agencies had their own hearing in Congress and their own review in the federal budget process. They also had a certain prestige position in the Washington complex. The White House social calendar included receptions for heads of independent agencies who, in number and by protocol, did not quite fit into the more traditional White House nights for the Judiciary, the Congress, and the Cabinet. In passing, we might observe that after the first Sputnik was launched, science, too, was included in the White House social calendar.

After nearly two decades of administrative experience, the government began to change the status of the host of independent agencies generated in the prolific days of the New Deal. Some were moved under the roofs of existing cabinet departments, others were pulled together in the new Department of Health, Education and Welfare, still others remained independent.

Although this analogy to the New Deal period can be overdrawn, the government still responds in similar ways in its search for new scientific and technical functions that are required, and in its experimentation with alternative formulas for getting these functions performed. Presently, there are many patterns. The structures of several cabinet departments have been modified to strengthen the role of science, and new policy groups have been established in the Executive Office of the President. In fact, since World War II, there have been two major modifications each year in the organization of science in government. All these actions have been motivated by a compelling desire to find coherent and orderly ways for undertaking a growing number of scientific activities. With the federal government supplying half of the total national expenditure in science and technology every year, it has not been easy to determine which scientific and technological activities should remain in independent agencies, which should be brought together within some existing cabinet department, and which should be the province of a new agency. Moreover, it has been difficult to determine the best way to formulate a government-wide policy for science, and the best way to incorporate scientific judgments and assessments in the formulation of national policies, both domestic and foreign.

Effective administrative structure

The present ferment and the present experimentation with the administrative structure are symptomatic of the search for ways to have the total job done most effectively, that is, government operations, and ways to make it possible for Congress and the executive branch to observe, understand, appraise, and govern. To the extent that Congress, on the one hand, and the executive branch, on the other, should, and must, control and direct all public agencies in fulfilling their responsibilities to the electorate, the search is bound to continue.

Part of the difficulty stems from the fact that the term "science" is used broadly and loosely within government and without. It includes, in this paper, technology. For it covers the broad spectrum of basic research, the search for new knowledge and understanding; applied research, the early attempts to convert science into technology; and, finally, engineering or development, the design and building of something new and useful. Unless "science" is understood in this broad context, important elements in the relationship of government to science are irretrievably lost.

The relationships of science to military operations and to such things as public health and agriculture are well understood. But in the contemporary age of the cold war—thaw, or not—science has intruded into diplomacy, disarmament, psychological warfare, and the development of economically depressed areas of the world. In these broader aspects of national policy, the scientist now joins hands with the government policy-maker, the educator, and the economist, in his concern with the total vigor and dynamics of the nation's economic and intellectual life. The new "age of science" has greatly enlarged the responsibility of government not only for the economic and social well-being of the nation but also for the health and progress of its scientific and technological achievement. Under these conditions, the scientific community, no less than the body politic, and the technical "braintrusters," no less than the man on the street, need to understand better the relationships of government to science.

SCIENCE IN GOVERNMENT OPERATIONS

In order to detail some of these relationships, it may be useful to describe the responsibilities of a few government agencies, both independent agencies and cabinet departments, which illustrate the variety of ways in which government and science interact.

The oldest postwar independent agency is the Atomic Energy Commission. It uses science and technology to meet the nation's needs within a vast area of responsibility from the development of nuclear power to the manufacture of nuclear bombs. The spectrum of its technical operation ranges from sophisticated manufacturing to the conduct and support of fundamental research. The size and scope of this effort illustrate only slightly the working-out of Parkinson's law; for aggressive technology requires an intimate association with basic research to uncover fundamental knowledge, and forge it, through engineering activities, into realities. Inevitably, the scope of the Commission's responsibility requires close relationships with industry and academic institutions not only in the physical and engineering sciences but also in the biological and medical sciences. Atomic energy has wide international aspects too, as the discussions on peaceful uses and suspension of nuclear tests serve to remind us. Thus the Commission's uses of science and technology cannot be confined to its own operations; they become inevitable factors in the formulation of domestic and foreign policies.

Chronologically, the next postwar scientific agency to be established was the National Science Foundation. The Foundation is charged with the major responsibilities for the support of basic research and education in the sciences, and this obviously cannot be done without the direct participation of the nation's universities. So it is that the Foundation has become greatly involved in, first, graduate education, and then, of necessity, with undergraduate education and even high school instruction and curriculum. The Foundation's interests overlap with those of the Atomic Energy Commission, the Department of Health, Education and Welfare, and with most federal activities in science and technology. The National Science Foundation is concerned also in the foreign field, as its roles in the Antarctic Program and the International Geophysical Year amply attest.

The nation's youngest scientific agency is the National Aeronautics and Space Administration. Historically, it is the successor to the National Advisory Committee for Aeronautics, an agency which provided the scientific and technological basis for the aircraft industry. The Space Administration's emphasis now, of course, is on space activities, but its operations likewise extend to industry and education. For whether we like it or not, we are heavily involved in a cold war rivalry for technological and scientific supremacy, and space has become a battleground, both in the technology of vehicles and the elegance of scientific experimentation.

Although the public responsibilities in atomic energy, basic research, and space exploration were placed in independent agencies, the lack of an obvious cabinet department to house them was in all likelihood the least compelling reason. More probable was the desire in Congress and the executive branch to deal with these areas directly in order to facilitate the identification of problems and objectives, to control budgets, and to more easily evaluate performance. In two instances—the Atomic Energy Commission and the National Aeronautics and Space Administration—Congress also established special committees of its own in response to a heightened public interest.

Science in the departments

A number of cabinet departments also have scientific and technical activities either in support of their own departmental operations or to provide government-wide technical services. The largest of these, of course, is the Department of Defense, where there is a critical need for maintaining close working relationships with the scientific community in order to stay in the forefront of weapons technology. Since time and again, scientific research has resulted in major improvements in weapons and the development of revolutionary new weapons, the Department of Defense must conduct and support a vast program of scientific research and development. Because of the size, character, and scope of its scientific operations, the way in which the Department of Defense uses and supports science and technology has a major impact on the economic and educational life of our own and other nations and on domestic and foreign policy.

The ever-growing role of science in the operations of the Department of Defense has brought with it many managerial changes in the last decade. The most recent reorganization resulted in the appointment of assistant secretaries for research and development in each of the military departments, the creation of the Advanced Research Projects Agency, and the establishment of a new post, Director of Defense Research and Engineering, to which Dr. Herbert York, a scientist, was appointed.

In the State Department, the post-Sputnik reaction led to the appointment of a science adviser to the Secretary and the re-establishment of scientific attachés in six major missions abroad. Similarly, following the meeting of the heads of government of the North Atlantic Treaty (NATO) Organization countries in December of 1957, a science adviser to the Secretary General of the NATO was appointed and a Science Committee established to advise the North Atlantic Council.

These changes in organization in the State Department reflect the enlarged role of science and technology in foreign affairs. We are witnessing their increased use on many international problems ranging from disarmament to world economic development. There is, furthermore, a rapid extension of scientific co-operation among nations. New programs for furthering scientific exchange are providing manifold opportunities for scientists to work together on common scientific problems.

There are other government departments, however, which have scattered scientific and technical activities, originally placed there for historical reasons, which are no longer of major departmental interest. These technical bureaus, despite a government-wide or national need for their services, do not always receive proper attention from their parent department. They have become, in effect, a bypass along the new roads the department has traveled. In the Department of Commerce, for example, the National Bureau of Standards receives over sixty per cent of its support from other federal agencies. The Weather Bureau has similar problems. Originally, the Weather Bureau was located in the Signal Corps of the Army, but it was later transferred to the Department of Agriculture. This might suggest that "weather" was more important to farmers than to the Army. When the Civil Aviation Agency was established in the Department of Commerce, the Weather Bureau was moved from Agriculture to Commerce. Was "weather" now more important to aviation than to farmers or the Army? Last year, aviation was moved out of the Department of Commerce into the new Federal Aviation Agency, but the Weather Bureau stayed where it was. Was "weather" now less important to aviation than it had been, but more important to commerce than to farmers and the Army? Obviously, the services of the Weather Bureau are important to many federal agencies and to the nation as a whole. If it cannot obtain proper support for its operational programs and research activities from a parent department, even though it serves other public agencies and numerous private institutions, its technical services are bound to be inadequate to the needs of all.

Similar observations can be made about the Coast and Geodetic Survey, another scientific and technical organization in the Department of Commerce, and the technical bureaus of the Department of the Interior. In the latter department, the Geological Survey, the Bureau of Mines, the Fish and Wildlife Service, and the Bureau of Commercial Fisheries all conduct scientific activities not only in support of the Department's own mission, but in order to provide other government agencies and private institutions in industry and education with scientific and technical data. In all these cases, adequate support must either be forthcoming from the parent departments, or these activities must be identified and studied to determine the feasibility of regrouping them.

SCIENCE IN THE WHITE HOUSE

A partial enumeration of the independent agencies and cabinet departments which use science and technology in the conduct of their operations indicates the scope and depth of the government's reliance on the nation's underlying scientific resources, and the interactions of science and technology with government operations. Obviously, broad guidelines are needed that adequately provide for dealing with common problems and competing needs, but which recognize that science and technology cannot be isolated from agency missions and operations.

Within the last two years, the President has taken three major steps to increase his use of advice and assistance in the development of policies governing the over-all relationships of government to science in the executive branch. He appointed a Special Assistant for Science and Technology, moved the Science Advisory Committee from the Office of Defense Mobilization to the White House, and created the Federal Council for Science and Techonology by executive order. These three actions reflect the seemingly paradoxical needs of the Presidency. The first two grew out of the President's need for informed scientific advice as free as possible from personal or departmental positions in the development of national policies. The third, recognizing that policy cannot be made in a vacuum, provided for the participation of policy officials from those government agencies with major operating programs in science and technology in the formulation and execution of policies.

Although the Special Assistant has unique responsibilities that differ from those of the Science Advisory Committee, together he and the Committee can serve the President in a number of ways. They can make intensive studies for him on various aspects of research and development programs. They can undertake other assignments for him of an advisory kind and provide answers to specific questions. They can mobilize the best scientific advice of the country and make recommendations with regard to ways in which American science and technology can be advanced by the government to serve the nation's interests. And they also can take the initiative in three specific policy-making areas: (1) in identifying and calling attention to the need for policy; (2) in securing the most competent examination of problems; and (3) in recommending alternative solutions.

A reading of the Committee's public reports on space, research policy, and science education reveals that a broader approach was undertaken than merely a merging of the views and objectives of federal departments and agencies. In trying to define what the government should do by direct action or stimulation and what the private sector of our society might be expected to do, these reports were a first attempt at dealing within the federal government with the totality of the nation's problems in these areas.

A large part of the work of the Special Assistant and the Committee, of course, is taken up with certain problems of national security whose major content is technological in character. In dealing with many of these problems, such agencies as the Department of Defense, the Atomic Energy Commission, and the State Department naturally will have their own departmental views. These individual positions may be valid from the point of view of the agency's statutory responsibilities and functions, but they will not always include an analysis of the technological choices open to the policy-maker which would permit an evaluation of possible alternatives. Since the Special Assistant and the Committee are not directly concerned with supporting agency objectives they frequently can provide the President with an opportunity to examine more than one course of action.

Special Assistant as liaison

The Special Assistant, although a member of the President's Science Advisory Committee and, in fact, its chairman, has individual functions to perform for the President that the Committee cannot undertake. He is in a position to inject daily the scientific and technological judgments required in formulating public policy. While the Committee advises, the coupling of this advice with administration policies is the particular responsibility of the Special Assistant. In addition, because he is in constant contact with government agencies and departments, the Special Assistant can present within the inner operations of the White House his own views on achieving integrated operations in science and technology.

Although the Committee and the Special Assistant provide advice on policy matters, a means for obtaining the participation of the operating agencies is needed both to develop and execute government-wide policies. The Federal Council for Science and Technology was established, also under the chairmanship of the Special Assistant, to perform this function. Now broad scientific and technical programs, such as oceanography which is of interest to the Departments of Defense, State, Commerce, Health, Education and Welfare, and Interior, the Atomic Energy Commission, and the National Science Foundation, can be evolved for the government as a whole. For the participation of all interested agencies is necessary in achieving coherent planning and mutual support. In this way oceanography can be viewed as a national resource, and the needs of this science, the needs of the agencies, and the needs of the nation can be considered as a whole. The interactions of the President's Science Advisory Committee and the Federal Council for Science and Technology, through their common chairman, the Special Assistant, may ensure that special fields of science vital to the nation can be given attention even though they do not fit into any specific department or agency. The procedures of the past, where every scientific field was viewed in terms of a narrow departmental need which, in turn, cut it into bits and pieces in the annual budget cycle, can now be modified.

WHERE DO WE STAND?

The ferment in the relationships of government to science and the rash of experimentation with the structure of science in government are producing a pattern that, although evolving slowly, is taking definite shape. The structure created at the White House level in response to the need for broad policies and direction will most likely continue.

The cabinet departments, too, are getting their houses in better order. Greater attention is being given to providing, at the secretary's level, for a policy official who can oversee all of the scientific and technology activities of a department and provide policy guidance for their activities. At the same time such an official can supply the secretary with technical advice in the formulation of broad policies. The existence of the Federal Council for Science and Technology has been an important factor in this regard, since it required the departments to send, as representatives, policy officials who could speak for their departments as a whole. Ultimately, it may be necessary in some of the older departments to have a special assistant for science and technology or an assistant secretary. For this pattern, which already exists in the Department of Defense with the appointment of a Director of Research and Engineering, has strengthened the scientific and technological operations throughout the Department. The Department of State, on the other hand, has special problems and has yet to evolve a satisfactory structure for properly relating science and technology to its responsibilities.

Among the independent agencies, the National Aeronautics and Space Agency and the National Science Foundation found early in their history that it was valuable to have technically trained people as directors. The Atomic Energy Commission has not had a technical person as chairman but has always numbered one scientist among its commissioners.

SCIENTISTS TO FILL THE NEW POSTS

Underpinning all the recent developments in the relationship of government to science, the creation of new scientific agencies and new government posts, are people. The many new jobs created in response to the increased importance of science and technology in our national life must be filled by people not only who understand the importance of science and technology to the nation but also who understand scientists and engineers.

Although it is not obvious that we will have enough scientists and engineers with the background and breadth of understanding about problems of government and public policy who can contribute the insight and understanding of their specialties, there are some hopeful developments. The scientific mobilization that occurred during World War II produced technical people who, by participating in new enterprises for the design of weapons, gained invaluable experience in industrial operations and in the operations of the military departments. They learned, as scientists learn, by doing and by experimentation of the interactions of science with government.

The current generation of young scientists and engineers is not gaining this experience and knowledge in anything like the same degree as the special environment of wartime provided. Nor is it adequately recognized as an important skill in our society to which our academic institutions should more fully respond. Yet people are being trained. One major training ground has been in the government itself. The Atomic Energy Commission, National Aeronautics and Space Agency, and the Department of Defense, to name a few agencies, are training hundreds of young people. The younger staff members at Geneva, who participated in the nuclear test cessation negotiations, for example, were given a rare and unique kind of training. Similarly, the laboratories in universities and industry that do work for the government provide an experience similar in many instances to that provided at Los Alamos and the Radiation Laboratory during the War. These are valuable potential sources of a special type of talent that the nation may soon find indispensable.

Although a more rational handling of the relationship of government to science will depend heavily on finding qualified people to fill the new jobs, it is well to remember that in 1930, hardly anyone was called a "labor relations expert;" now every industrial company has one. The climate that will pull well-trained scientists and engineers into public service is dependent upon an increased understanding of the role of science in society, and this understanding is growing.

From time to time in the history of our nation, progress often has depended upon new approaches to problems and issues. In the last few decades this nation took far-reaching actions in the economic and social fields. At present, national attention is focussed on science. History would again seem to indicate that it is no time for despair, but, rather, a time of great opportunity and challenge.

Influence of the Past: An Interpretation of Recent Development in the Context of 200 Years of History

By A. Hunter Dupree

Abstract: Although the eighteenth century spoke bravely of internationalism in science, its scientists were put under heavy pressure by the rise of the nation-state. Ease of communication in the nineteenth century made it seem as if science were the product of an international community, while at the same time science was becoming more dependent on nationally divided institutions for support. When in the twentieth century the free flow of information ceased, it became clear how closely science was tied to the various national units. The same pattern can be traced in the relation of basic to applied research. The eighteenth century saw the two brands of research as closely linked, but in the nineteenth specialization drove the two activities wide apart to form separate traditions. In the twentieth century military problems brought basic and applied research together again. Likewise the same pattern can be traced in the evolution of institutions within national units. The eighteenth century could envision comprehensive scientific institutions, but the nineteenth century created government, industrial, and university research institutions in separate compartments. The problems of the mid-twentieth century are defined by the basic relations among the various institutions within the nation, the very relations which the peculiar characteristics of the nineteenth century had obscured.

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SIR Joseph Banks, President of the Royal Society of London, was writing to his counterpart of the Institut de France in 1804, at the time the war had recently been renewed. He insisted that it had "always been my wish to promote the scientific intercourse among Nations—notwithstanding any political divisons which might subsist between them." Yet the hard facts of the struggle with Napoleon put his ideal under strain. If "I cannot maintain a correspondence with learned Englishmen in France, without being accused of employing them for political objects; and if gentlemen of known reputation and honour cannot visit your Country for the purpose of giving and receiving scientific information, without being exposed on every turn of public affairs, to the vile imputation of acting as Spies, . . . it will be impossible for me—either to preserve a constant intercourse of good offices between the Men of Science in the two Nations; or to insure for learned Frenchmen, who may come to England, those marked attentions and civilities, which it is my wish that they should receive in time of war as well as peace." [1]

The scientist of the middle of the twentieth century will smile at this lofty ideal of the age of the philosophes. He well knows that national interest has become a paramount force in the affairs of science, that continued intercourse between the scientists of warring nations is beyond the imagination of practical men. Even while he smiles, however, the scientist harried by security checks and the demands of military research looks back at the eighteenth century with a feeling that it was a paradise for science and a model which might apply in the nuclear age if only men would be reasonable. Science knows no nation, and any connection between national interest and science, where it cannot be ignored, is a matter for shrugging apology.

THE RISE OF NATIONALISM AND SCIENCE

Admiration for eighteenth-century internationalism in science usually leads to the quotation of sentiments such as those of Sir Joseph, which can be found in great numbers in the documents of the years between 1770 and 1815. Nevertheless one must go on to ask: Why does the ideal of the complete internationalism of science become so explicit in the very years that the modern nation-state was taking shape? Is there no historical connection between science and the rise of nationalism? Is science, unlike art, literature, religion, politics, and philosophy, completely divorced from one of the primary forces at work during the past two centuries? The very explicitness of the pronouncements of the men of the era of the American and French Revolutions is a sensitive indication that the rise of nationalism was in fact straining the internationalism of science. Sir Joseph Banks was eloquent about the rights of scientists in wartime precisely because he was under attack in London by pamphlets, aptly signed "Misogallus," which accused him of being a collaborator with the French. And he was writing to the President of the First Class of the Institut de France not because communication was really so free between the two warring countries, but rather because the unsporting Napoleon had thrown so many vacationing Englishmen, members of the Royal Society as well as others, into jail with the collapse of the Peace of Amiens.

The difference between the eighteenth century and the twentieth lay not so much in the absence of a connection between science and nationalism as in the fact that science had not yet made itself practically relevant to the affairs of nations. The eighteenth-century man took an intimate connection for granted. The great exploring expeditions of the late eighteenth century, the very ones which developed the scientific corps of naval expeditions to a high peak of specialization and efficiency, were inseparably mixed in their objectives. Louis Antoine de Bougainville went around the world in the joint service of science and France. Captain James Cook combined his objectives in the same way. For explorations of such scope, both military support and a nationalistic motive brought success where science alone was insufficient. Hence the eighteenth century knew well that an intimate connection between science and the nation-state existed.

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[1] Joseph Banks, London, to the President of the First Class of the National Institute of France, January 30, 1804, manuscript in the historic letter file, Gray Herbarium, Harvard University, Cambridge, Mass.

Illusion of internationalism

The insight of the eighteenth century was lost, however, because of the peculiar experience of the nineteenth century. The general peace which lasted from 1815 to 1914 provided the backdrop for scientific accomplishments of unprecedented splendor. It was the century of the rise of chemistry. It was the century of Charles Darwin and the coming of age of biology. Even physics, seemingly so well worked out, moved into a new period of creativity before 1914. The great accomplishments of the nineteenth century seemed to be the work of an international community. None of the Western nations had a monopoly on science, and there was little to suggest that the two overshadowing phenomena of the century, the rise of science and the rise of an exclusive nationalism, had anything to do with one another.

The peculiar characteristic of Western civilization in the nineteenth century which masked the connection between science and nationalism was ease of communication. Not only did the fruits of technology provide steamships and railroads to make communication easier, but the possibility of the migration of people, and hence of knowledge, from one part of the world to another came within the reach of the masses in Europe. This easy flow of people and ideas over national borders, a flow which had much to do with building the United States into a great and populous nation, gave an illusion of the existence of an international community for at least the Western world. Science flowed over national borders even more easily than other kinds of ideas. Those who lament American backwardness in science in the nineteenth century often overlook the fact that the very ease of communication with the scientific centers of Europe contributed to seeming backwardness. Alexis de Tocqueville is often quoted on the lack of American interest in abstract science. He is less often given credit for pointing out that the availability of European research removed the necessity of the repetition of such work inside the American national unit.

The ease of international exchange of scientific information all over the world produced such striking results that most scientists made the unspoken assumption that science was exclusively an international phenomenon completely divorced from the interests of nation-states. This assumption made it easy to overlook a profound dependence of science on the nation-state which was growing up all during the century almost unnoticed. Science is dependent on the institutions which house it and give it support, however much individual reputations outshine their institutional setting. During the whole of the nineteenth century after 1815 the support of science polarized around national institutions. Even the universities, German, British, and American, increasingly differentiated themselves from one another along national lines. Certainly government efforts in science, which were reaching huge proportions before 1914, were very much tangled with considerations of national interest. This connection did not worry anyone very much, however, because national institutions, even government ones, participated in the free flow of information which fed science such a rich fuel.

Impenetrable barriers

The first World War gave science a considerable jolt, and patterns of a close relation between science and weapons research emerged. The generation of scientists which fought the war nevertheless concluded from their experience that the national implications of scientific research belonged to that one horrible episode alone, and they emphasized a more self-conscious international feeling than they had before the war.

George Ellery Hale shifted his attention from the National Research Council, which he had been instrumental in creating for the purpose of the United States war effort, even before hostilities ceased. He saw the International Research Council, to which he turned his energy, as the logical institutional development for the postwar world. Hence American scientists shared little of the isolationism of their fellow citizens during the 1920's, and some of them could believe, in the interwar eddies of history, that a true international community of science not only existed but provided all the support which science needed.

But under the blows of depression, totalitarianism, and war the illusion of a true international community for science vanished, along with all sorts of other aspirations for a true international community. A lack of free flow of peoples and information became a major characteristic of the mid-twentieth century. Speed and ease of transportation were irrelevant as national boundaries became impenetrable barriers to the movement of ideas. The scientists were rightfully dismayed, for the feeder of science in its great age, the nineteenth century, was gone. At the same time, they became aware of the hold that their national institutions had on them.

Nations such as the United States found uses for scientists which were intimately connected with survival. Much as they regretted the loss of the old feeling of world-wide community, many saw that the national interest in science represented new opportunities as well as inconvenient restrictions. The rapidity with which this adjustment was made is one of the great themes of the history of science during the second World War.

OTHER HISTORICAL DILEMMAS

The scientists who had reached maturity in 1939 had to make other adjustments. These adjustments were necessary in realms where history had placed them in situations that were completely obscured by the scientists' own ideas about their place in history. Just as internationalism in science had, like free trade, seemed the natural order of things because of unique factors operating in the nineteenth century, the relation of applied to basic research presented a historical problem to the generation entering the war. They were also ill prepared to understand the division of responsibility among the institutions in their own national units. The movements sketched in the ideas concerning nationalism and science can also be traced in the relation of basic to applied science and in the institutional development within the nations.

Applied versus basic science

In the eighteenth century, while science was largely in the hands of the genteel and technology largely in the hands of artisans, the happy lack of specialization allowed a free movement between basic and applied research. Alexander von Humboldt, Count Rumford, and in America Benjamin Franklin and Thomas Jefferson all showed the ability to combine an interest in science as a system of philosophical ideas and as a tool of men. If the eighteenth-century accomplishment in science as "ideas" overshadowed the practical results of science as "a tool of man," the philosophes still kept the two enterprises in close range of one another.

The order of the nineteenth century was specialization. The sciences themselves fragmented into specialties. In addition, the spectrum which runs from basic to applied science also became specialized. It would be easy to compile long lists of the accomplishments of basic research before 1914 and then match them by equally impressive and unprecedented achievements in technology. Even though basic science played an increasing part in these technological marvels, quite clearly two different groups of men wrought the discoveries in basic and applied research. So far apart did these two groups drift that they lost touch with one another. The basic scientist reserved his feeling of fellowship for other basic scientists like himself. He felt independent of the man concerned with practical problems and superior to him. And the results on both sides of the gulf were so spectacular that the parochialism of both basic and applied scientist became entrenched. In the last third of the nineteenth century a fundamental change took place, that is, the systematic harnessing of science to the problems of technology. Applied science began to earn its reputation as the servant of man. Yet the two separate groups tended to go their separate ways as long as they had reasonable latitude in choosing the lines of research they wished to follow.

The experience of the first World War suggested that a marked acceleration in applied research, for instance concerning weapons, would quickly lead to questions for which basic research had no answers. At the same time the urgency of applied research drew off potential manpower from the supposedly cloistered haunts of basic science. The policy of the postwar period attempted to perpetuate the separateness of the two guilds. Occasional laments in the 1920's and 1930's indicate that thoughtful men feared that basic research was somehow losing out to applied research, but they did not explain clearly how advance in the applied area could detract from basic research, and they did not do very much about it. Weapons research was generally so unpopular a field in the interwar period that it provided little impetus to applied research and almost none at all to basic science.

On this theme too the generation of scientists who had matured by 1939 found that they had to make most difficult adjustments. Many of them were proud of being basic scientists, the choosers of their own problems by criteria fiercely divorced from practical considerations. Yet in the war effort they found their skill as scientists stretched to the utmost in solving problems which were, undeniably, terribly practical in their immediate results. What they had confidently left to the engineers before the war now became their business. And the engineer found that he was dealing with basic questions. All along the spectrum from basic to applied the gradations between the bands became blurred. The myriad definitions of basic and applied research which have sprung up since the second

World War have only emphasized the impossibility of drawing a line. And the basic scientist can no longer avoid many of the dilemmas which the admitted practicality of his work entails.

National institutional structures

A third line of historical development has played directly into the confusion of the scientific scene since 1940. Because many scientists were not aware of the close tie between science and their own national units, which the support of science entailed, they did not understand clearly the internal institutional organization in which they worked. In the United States especially the institutional structure was hard to understand because it was less centralized than in many of the other great scientific nations. Here again the eighteenth century and early nineteenth century had produced plans for the organization of science. Jefferson, John Quincy Adams, and others in the early Republic, had clearly envisioned a national university and a full set of scientific institutions for the country They failed not in conceiving the structure of the institutions but in reducing their plans to working organizations. Their successors, who incidentally profited unawares from the great peace of the nineteenth century, created institutions as they needed them without asking ultimate questions about their nature and orientation. Great government scientific research agencies arose under the guise of mundane functions such as a Depot of Charts and Instruments or to meet the need for medical care among merchant seamen. Even the American university, which rose rapidly as the home of scientific research after 1870, asked few questions about its relation to other scientific institutions, such as those in the government, which were actually senior to it.

Shortly after 1900 a division of labor among the scientific institutions of the country became firmly entrenched. The government limited itself to those areas of applied research which aided its own operations or which served some special segment of the population unable to provide for its own scientific service. The farmers were the most conspicuous example of the special segment. The government had no responsibility for basic research except in a few enclaves like the Smithsonian Institution, and indeed, congressmen asked very embarrassing questions when an agency such as the Biological Survey launched a program too evidently basic science. And even though government science prospered, the men who chose its service seemed somehow less pure than their confreres elsewhere. Industry looked neither to the government nor the universities for research, but began to develop its own laboratories. While one or two pioneers allowed considerable latitude to their scientists, the universal rule in industrial research was to stick close to the problems related to the products of the company. The industrial research laboratory developed a world of its own, with its own traditions.

Universities foster basic science

The universities, formed between 1870 and 1900 and financed to a new state of affluence in the twentieth century by the largesse of great wealth, were the keepers of basic research. It was their business to foster science independent of the vulgar uses which filled the minds of the government or industrial scientist. In their graduate schools, the universities trained scientists to a new pitch of specialization, but those who went into government and industry to undertake applied science usually had little effect on the academic institutions in which they had been trained. Indeed the scientists who dwelled in the halls of basic research had little sense of being encased in an institution at all, much less one limited by its nationality. The instances were few where equipment was so expensive and complicated that the scientist had to think in large scale terms as an institutional administrator rather than as an individual.

American society possessed no mechanism for adjusting the relations among science as housed in the government, industry, and the universities. As long as the comfortable division of labor seemed to work, no one felt the need of adjustment. Industry and government accepted their role; the universities gloried in theirs. Co-ordination of their activities seemed not only unnecessary but also dangerous. The scientist of 1939 accepted this division as the natural order of things. He took his institutional arrangements so completely for granted that he tended to deny he had any. He was a free agent in the universe, and anything modifying his complete and untrammeled individualism was the worst of disasters.

New patterns emerge

In the despised haunts of applied science, however, new patterns had long been emerging which changed the scientist's relation to his underlying institutions and indeed his whole way of conducting research. As early as 1830, the Department of Agriculture stopped trying to be a university organized by divisions of knowledge and began shaping itself around the problems it needed to solve. Thus discovery did not come out of nowhere. The unsolved problem came first. Then specialists from various fields worked until a solution was found. Specialists remained in their own fields, but the problem brought them together with other kinds of specialists in a team. A generation later industry took up the same general approach. The results of this change of method are spread across the face of the twentieth-century world. But the scientist was no longer the lone hero, choosing his problems and assaulting by his unaided individual wit. The problem approach and team organization were the engines which revolutionized the relation of science to the military at the outbreak of the second World War.

In all three of the lines briefly sketched above there was plenty of fuel for the disillusionment of the scientist who had reached maturity by 1939. He thought science was international, soaring quietly above the fevered interests of nations. He thought basic science was unconcerned with the uses to which it was ultimately put. At least that was none of his professional affair. He thought that his position in the institutional structure of his nation was either unimportant or nonexistent. The world since 1939 has seemed triply horrid.

Had the second World War joined the first as a temporary disruption of these three views of science in a vacuum, the postwar organization of research, and with it the life of the individual scientist, might have reverted to its nineteenth-century mold. Yet the postwar scientific scene has shown no sign of resembling even remotely the dominant beliefs of the 1930's. A new age has arrived which seems to be but a beginning.

THE TWENTIETH-CENTURY OUTLOOK

Internationalism in science in the mid-twentieth century is not a description of an actual state of affairs but rather the glimmer of a distant goal. The free interchange of information which wrought such miracles in the nineteenth century would do good service today, but the factors which made it possible have vanished. The national unit, with its military stake in science, is more robust than the international community. Any increase in flow not only requires painful negotiations but is tied to the cause of internationalism in general. Science is not a separate sphere which can be gaily international while the world remains sundered. Scientists' hopes lie with those of the rest of mankind.

The harnessing of whole national scientific establishments to military applications pulled all scientists, whether they tagged themselves as basic or applied, into the same network. The most seemingly esoteric of basic discoveries are credited in advance with the possibility of practical applications. On the other hand the most practical of problems call for theoretical solutions far beyond the dreams of basic scientists. The world is no longer amazed when a formula becomes a bomb. And physicists working on bombs can complain that the mathematicians who furnish them formulae only work on the easy questions. Nothing in the postwar world, certainly not the thermonuclear bomb or the penetration of space, has lessened this interwoven pattern.

In the new institutional patterns the other two changes are clearly reflected. The national research establishment has laid bare its underlying structure. The neat, logical division of labor among the government, industry, and the universities has broken down. In its place stands a single structure linked together by a network of grants, contracts, and research corporations. The government, by the very nature of its interest in survival, is the major component in the new structure. Both the universities and industry look to it as the prime source of research funds and clues for the direction which research must take. Yet the government has not simply taken over science in the United States. The phrase "federalism by contract" more accurately describes the state of affairs. Moreover, the problems of adjustment among the institutions of science are impossible to ignore.

None of the problems facing science in the postwar world can be solved by pointing to the limitations of the understanding of the generation which went into the second World War. In a very real sense that generation acted in response to the underlying historical realities instead of to their partial analysis of the place of science in history. In the free world they quickly chose a more limited allegiance than some distant and unembodied internationalism. They immediately turned brains versed in theory to practical problems which were relevant to the immediate crisis. They began changing their institutions and their relation to the government even before the outbreak of hostilities. Although some in 1939 believed that science could go its way without facing the problems of society, and although some still talk as if they wish it were so, scientists have not persisted in acting as if they live in a special vacuum.

It should help in perspective if not in finding solutions to realize that science has never operated in a vacuum according to rules of its own, however much the nineteenth century made it seem to do so. The major characteristics of the postwar scientific establishment have been many years in the making. Society faces a serious problem in digesting science as an integral part of its structure. But this is not a completely new problem or one more intractable than many others. Scientists can still seek solutions which recognize internationalism, the pursuit of knowledge for its own sake, and individualism, but they must seek them in the same context as the rest of mankind.

The Need for a Department of Science

By Hubert H. Humphrey

Abstract: The Senate Committee on Government Operations, in which Senator Humphrey has taken an active role, has concerned itself with the need for a science department in the government. A Department of Science and Technology they have found would be a very necessary and essential adjunct to our governmental system. Its main purpose would be the co-ordination and centralization of the now dispersed federal scientific activities. The new Department would promote the clear-cut responsibility and authority in government science needed for the enhancement of our national security. Yet the reorganization of the existing science units contained in various departments and agencies of the government, has met strenuous opposition from sources in the Administration. In addition to the proposed bill providing for the creation of the Department of Science and Technology, the Senate Committee has initiated another bill now before Congress which provides for a Commission to study the various problems connected with the creation of such a science department.—Ed.

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THE *First Finding* of the original Hoover Commission on Organization of the Executive Branch of the Government, submitted to the Congress in February 1949, and upon which the reports of the Commission were based, was that "the executive branch is not organized into a workable number of major departments and agencies which the President can effectively direct, but is cut up into a large number of agencies which divide responsibility and which are too great in number for effective direction from the top."[1]

The proposal to create a Department of Science and Technology within the executive branch of the federal government, which I first proposed more than two years ago, was based upon that sound principle of government. This was three months before "Sputnik" brought the Congress and the American people to the realization that extensive reorganization of federal science programs was necessary if we, in co-operation with other free nations of the world, were to reassert our leadership in the field of science.

During the last decade the United States and the entire world have been caught up and swept along by fantastic advancements in the scientific world. The executive branch of the federal government and the Congress have, fortunately, taken the initiative, through more or less haphazard and unrelated actions, to provide a spearhead for progress to meet the challenge of greater technological advancements. It is imperative, however, that this country continue to be the motivating factor and maintain its lead in scientific development, and be prepared to carry on a comprehensive and constructive program to meet the challenge of world communism. To continue this leadership, a complete re-evaluation must be made of present federal operations in science, research, and technological fields, and effective steps taken with the least possible delay to co-ordinate and improve the existing organizational structure of our government which carries on these functions.

DEVELOPMENT OF SCIENTIFIC AGENCIES

To meet the many problems as they arose, new agencies were created and efforts made to bring about improvements in the federal science programs. The Congress has provided necessary funds as required in support of such programs, and I am confident it will continue to do so. Among the most progressive steps taken to meet the objectives in the civilian science field were the appointment of a Special Assistant to the President for Science and Technology; the creation of the President's Advisory Committees on Science and Technology and on Government Organization; the establishment of the Interdepartmental Committee on Scientific Research and Development, later superseded by the Federal Council for Science and Technology; the extension of the authority of the National Advisory Committee for Aeronautics through the creation of the National Aeronautics and Space Administration; and the establishment of Committees on Science in the House and Senate.

While these steps have been helpful and undoubtedly are necessary actions in pursuit of the over-all objective of efficient government operations in the field of science, further consideration must be given to still existing inadequacies not only in these agencies but in other areas which must be co-ordinated with operations of a similar or related character and reorganized under more cohesive administrative controls.

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[1] Commission on Organization of the Executive Branch of the Government, *First Finding*, a report to the Congress, February 1949 (Washington: Government Printing Office, 1949), p. 3.

The basic approach to reorganization recommended by the Hoover Commission was prophetic in regard to the present status of science operations of the federal government. There are approximately fifty agencies which perform some science and research activities having direct application to the over-all science program of the government. In addition to the four Presidential advisory groups dealing with science, there are three major independent science agencies or commissions and more than forty components of other departments and agencies which operate science and engineering programs supposedly related to the major departmental mission for which the departments were established, or for which special agencies were created.

Some of these internal agencies, such as the National Bureau of Standards and the Office of Technical Services in the Department of Commerce, are performing services not directly associated with the primary mission or even related functions of the departments in which they are now located. Consequently, they do not receive the technical administrative direction necessary to insure operation at their highest potential. On the other hand, some of these internal agencies, which include the National Institutes of Health, the Agricultural Research Service, the Advanced Research Projects Agency, and the Science Advisor to the Secretary of State, perform services which most authorities on science programs of the government feel do have direct application to the program areas of the respective departments in which they are located and should retain their present status.

CO-ORDINATION OF FEDERAL ACTIVITIES

From the beginning, the Senate Committee on Government Operations, on which I serve, in its studies of the problems, studiously avoided involving the civilian science activities with the applied science and research programs of the military departments which pertain to national defense and come within the jurisdiction of other committees, or with advisory or co-ordinating agencies or officials within the Executive Office of the President performing high administrative and policy functions under the direction of the President.

The major objective of the science and technology act of 1958, which I introduced as Chairman of the Subcommittee on Reorganization and International Organizations, was to bring about, under a Secretary at the Cabinet level, co-ordination and centralization of those federal activities now scattered among existing departments, as well as those which had grown haphazardly following World War II in the field of civilian science.

It was proposed, as an approach to these problems of reorganization and improvement of science programs and for the purpose of eliminating duplications and waste of scientific, technological, and engineering manpower, to (1) promote closer co-operation between federal agencies which carry on science activities; (2) improve, co-ordinate, and expand the federal science information and documentation programs; (3) afford adequate federal support for education in physical sciences and engineering; (4) encourage the establishment of institutions in areas of broad basic scientific activity, such as oceanography, meteorology, etc.; (5) to establish and maintain science programs abroad through the use of foreign currency credits; and (6) to improve and elevate the status of science within the legislative branch by creating standing committees in the House and Senate which would have direct jurisdiction over civilian science programs of the federal government. Thus full Congressional evaluation of all such activities and the relationship of these widely scattered agencies to similar programs and operations would be assured. All of these committee recommendations, except the proposal to create a Department of Science and Technology, have been activated in varying degrees, although continued re-evaluation and further appropriate actions to fully implement these programs are necessary to achieve fully the objectives sought.

A new department

The Committee on Government Operations was confronted from the beginning with four primary problems: (1) Would a Department of Science and Technology afford the necessary administrative controls and science leadership required to correct existing deficiencies in federal operations in science and research; (2) Would such centralization of functions and supervisory control afford an answer to the multitudinous duplications and waste that presently exist in our federal science programs; (3) Would it in any way impair any of the independent activities and basic objectives of certain of the specialized agencies which had been established by the Congress and the President, or cause any disruptions in existing science and research programs; and (4) If a Department of Science were to be created, which independent agencies, or components of existing federal departments, should be incorporated within such a department?

Provisions of the bill

In an effort to avoid disruptions of existing programs and to ensure that full appraisal of these problems could be made before any permanent reorganization was proposed the bill introduced in the Eighty-fifth Congress, and again in the Eighty-sixth Congress, to create such a Department of Science and Technology merely provided for grouping of federal science agencies. The proposed Secretary of Science would have administrative control over certain of the independent agencies: the Atomic Energy Commission, the National Science Foundation, and the National Advisory Committee for Aeronautics, now the National Aeronautics and Space Administration; and certain components of existing departments: the National Bureau of Standards and the Office of Technical Services of the Department of Commerce, and the science functions of the Smithsonian Institution. The bill provided for no change in the basic statutes governing the operations of any of these agencies. Rather it was designed to promote more administrative efficiency in the government's scientific activities and to provide a framework for better co-ordination of all scientific functions. The committee proceeded under the assumption that, if a department was established, internal reorganization could be effected as experience and operation needs required.

There have been other suggestions relative to the creation of a Department of Science, which involve entirely different approaches. The proposal outlined by Dr. L. V. Berkner, President of Associated Universities which operate the Brookhaven National Laboratory for the Atomic Energy Commission, has received a considerable amount of support from scientists and administrators of science programs. Under Dr. Berkner's plan, the National Science Foundation, the National Aeronautics and Space Administration, and the Atomic Energy Commission would retain their independent status as specialized agencies, while the proposed Department of Science would set up centralized components in various areas of physical sciences, such as oceanography, meteorology, climatology, and standards.

OPPOSITION TO REORGANIZATION

In recognition of the varying points of view held by those who were active in the federal science program, the staff of the Senate Committee on Government Operations, in carrying out the committee's directive to draft suggested legislative proposals to meet the needs of the government, made numerous contacts with officials carrying on various science programs. Except in one or two instances, there was strenuous opposition to any change in the present organizational structure.

The Hoover Commission in its concluding report also warned of this type of resistance to any proposed reorganization efforts, as follows:

> It is natural to expect vigorous opposition to reforms from agencies and groups, each of which approves heartily of reorganizations that do not affect its own immediate interests. The Congress must be prepared to accept this fact and give careful attention to the validity of arguments of those who would seek to escape reorganization, as many have so successfully done in the past.
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> . . . While honest differences of opinion certainly may exist as to the best method of accomplishing any particular reform, such differences should not be accepted as valid arguments against the desired objective. Once the practice of exempting certain agencies and excepting particular functions has begun, the chances of achieving substantial improvements in the efficiency of the Government will speedily diminish.[2]

At the hearings which followed the introduction of the bill proposing the creation of a Department of Science and Technology in the present Congress, all officials of the federal government who testified were vigorous in their opposition to any legislation to bring about better co-ordination of science activities, and insisted on maintenance of the status quo.

Political interference with science

Objections were also expressed on the premise that it would inject politics into science activities; that the establishment of a Department of Science and Technology would bring scientists under bureaucratic control, which would be detrimental to the performance of the functions presently vested in various federal agencies.

This fear apparently exists only with reference to the brand of political overlordship emanating from an executive department; the brand of political control which could issue from the Executive Office of the President, and it seems to me most inconsistently, is apparently regarded as a benevolent variety of political control. This attitude ignores the fact that the President enjoys complete authority over the administrative functions of the executive departments, all independent agencies, and over the policies he establishes within his own office. In all instances, the appointees are named by the President. Those whom the President sees fit to vest with wide authority in the Executive Office of the President are his own selections, while those who are at the head of the departments and agencies must undergo close scrutiny as to their qualifications in their respective fields of operation before they receive Senate confirmation.

With particular reference to political interference, it is important to remember similar alleged forebodings in relation to the establishment of the present Department of Health, Education and Welfare, under which socialized medicine and federal control of education in the United States was said to be inevitable. Nothing could have been further from the truth. It is a matter of common knowledge that, in the world-renowned laboratories and clinics of the National Institutes of Health of the Public Health Service, an arm of the Department of Health, Education, and Welfare, the freedom of the research scientist to pursue his investigations, wherever they may lead, is a zealously guarded personal prerogative. Of course there is political control, but a control executed by a government of laws; a control which has enabled this country to take a pre-eminent position in today's world.

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[2] Commission on Organization of the Executive Branch of the Government, *Concluding Report,* a report to the Congress, May 1949 (Washington: Government Printing Office, 1949), pp. 47–8.

It is necessary to remember, too, that a Department of Science and Technology would be new in name only; its constituent agencies would be taken from those which now administer the relationship between science and the government. The need for the new Department exists because these relationships are being administered without the co-ordination so urgently demanded by the exigencies of the time. This objective is in accord with another recommendation of the Hoover Commission, that "The numerous agencies of the Executive Branch must be grouped into departments as nearly as possible by major purposes in order to give a coherent mission to each department."[3]

Progress in other areas

It is fortunate that real progress is being made in some scientific areas, as originally proposed in the Science and Technology Act of 1958, despite the present continuing disorganization. An illustration of this is afforded by Title IX of the National Defense Education Act of 1958 of which I was also a sponsor. This legislation requires the National Science Foundation to form a Science Information Service and an advisory Science Information Council to provide for the accumulation, translation, indexing, retrieval, and other services necessary to promote more effective availability and dissemination of the scientific literature of the world. Pursuant to this enactment, the Science Information Service was established and the Science Information Council was appointed.

Further, the National Science Foundation announced formation of the Federal Advisory Committee on Scientific Information, composed of representatives of seventeen federal agencies engaged in scientific information activities, the purpose of which, according to the Foundation, is to provide "an integrated approach to the Government phase of the intensified national science information effort." Also, the National Science Foundation supported the establishment, within the National Bureau of Standards, of the Research Information Center and Advisory Service on Information Processing, primarily to encourage automation in the processing of scientific information. However, the progress thus far made, through the information program and other programs now actually under way, toward effective co-ordination of all the government's scientific interests is grievously outweighed by that which still remains to be done.

Operations differ

One of the most persistent arguments constantly advanced against the creation of a Department of Science and Technology is that the programs, responsibilities, objectives, and operations of each of the federal agencies engaged in science activities are different in character and have special fields of responsibility, which must be rigorously preserved. Those who make this contention insist at the same time that there must be a uniform science policy. Thus, while contending that a Department of Science would drain off manpower and scientists now engaged in the various programs operating within their own areas of activity, the fact that a centralized agency would bring all related science activities into closer contact and tend to eliminate the existing competition for and wastage of scientific and technological manpower, is completely ignored. The heart of this argument, as I understand it, is that the only way to ensure co-ordination is to continue the multitude of independent agencies operating in specialized fields and to have superadvisory or co-ordinating scientific groups within the Executive Office of the President act as their guardians.

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[3] Commission on Organization of the Executive Branch of the Government, Recommendation No 12, *Report on General Management of the Federal Government,* February, 1949 (Washington. Government Printing Office, 1949), p. 34.

Clear-cut responsibility

Those who place their reliance on coordinating devices such as interagency committees for improving communications and for providing stimulus to government science activities fail to recognize the built-in limitations of these approaches. By their very nature interagency committees cannot be creative except at a very low level of operation They are inevitably restricted by the views of the most pedestrian and unimaginative members. Their product will almost certainly be a kind of lowest common denominator of their combined ideas. What is needed is dynamic, forceful, and continuing leadership which could best come from a cabinet department with clear-cut responsibility and authority in the field of science and technology. For it seems to me that the present structure of the federal government in other areas, with the programs devised and implemented by the concerted action of the Chief Executive and the Congress, is the obvious answer.

STUDY COMMISSION

At the hearings held before my subcommittee in April of this year, a number of witnesses suggested that, in order to solve some of the above outlined problems, a Commission patterned along the lines of the Hoover Commission should be established to make a study as to whether or not a Department of Science and Technology should be created. If such a department were found to be desirable, the Commission should then recommend to the President and the Congress which functions now being performed by other departments and agencies of the government should be transferred to such a department. It was suggested that the Commission should be composed of eminent authorities in the field of science, representatives of the federal government agencies which were engaged in basic civilian science activities, and members of the legislative branch of the government.

At the conclusion of these hearings, the staff of the Committee on Government Operations drafted a bill providing for the establishment of such a Commission. The bill was introduced as Senate bill 1851 on May 5, 1959. It was the view of the Committee that its provisions were broad enough to authorize the Commission to study: the need for strengthening American science and technology, one of our esential resources for national security and welfare; the reorganization of the existing departments and agencies to promote better centralization and co-ordination of federal science programs and operations; and the formulation of effective policies for training, recruiting, and utilizing scientists and engineers.

Witnesses testify

At the concluding hearing of the Committee, which was held on May 28, 1959, witnesses were invited to direct their testimony not only to the bill proposing the creation of a Department of Science and Technology, but also to the proposed establishment of a Commission on a Department of Science and Technology. Some of the spokesmen for the administration who testified at these hearings contended that there was no need for legislative action and opposed both proposals. Their opposition to the creation of a study commission to determine what might be done to improve and co-ordinate federal civilian science programs was based on the contention that the President's Science Advisory Committee and the Federal Council for Science and Technology, both of which were then headed by Dr. James R. Killian, Jr., would provide all the necessary information upon which the President could take appropriate action to bring about governmental reorganizations to correct deficiencies in present operations.

Government witnesses who opposed the creation of a commission to study these problems repeatedly placed heavy emphasis upon the personal ability and qualifications of Dr. Killian and the confidence placed upon him by the President and the scientific community. Yet, at that very moment, Dr. Killian's resignation had already been submitted to the President. Without reflecting in any manner upon the qualifications or potential leadership of any of the science advisors to the President, I think Dr. Killian's resignation makes it more imperative than ever that Congress reassert its legislative prerogatives in the field of science. An accountable officer, whether a member of the Cabinet or otherwise, should be made responsive to the needs of Congressional committees which are attempting to eliminate the existing deficiencies in the science program of the federal government.

A real need exists

An analysis of the testimony of spokesmen who were representing the President at the hearing, will force the inevitable conclusion that it is the desire of the present administration to center within the Executive Office of the President all control over civilian science operations. If these recommendations are accepted, they would deny to the Congress access to information which is essential to the legislative branch if it is to perform its normal constitutional functions. It tends to pre-empt the scientists who are thoroughly informed and conversant about governmental science operation for the benefit of the President alone, and effectively prevents the Congress from obtaining information from qualified experts in the fields of science.

It was the conclusion of the Committee that a real need exists for a bipartisan Commission to study the problems related to the proposed creation of a Department of Science and Technology. Such a bipartisan Commission could find an equitable solution to the present chaotic conditions of federal science. The bill to establish such a Commission is now awaiting approval by the Senate and the House of Representatives next session.

It has been stated by many who oppose a Department of Science that scientists must be free to learn through observations and contacts with other scientists in order to cope with the problems of science in their own fields. It would be hard to imagine a better means of effective observation than to have the basic science programs co-ordinated under central administrative control, where daily observations and contacts could be made as required, without having to make separate approaches to a number of individual officials and agencies carrying on related science programs with no central co-ordination of policy or operations.

Decision with respect to a creation of a Department of Science and Technology, or of another kind of authority of no less eminence, is, in my judgment, the overriding issue and the obvious need. Fettered leadership, which experience has so often demonstrated, is all that can be expected from makeshift, compromise arrangements devoid of statutory authority and totally dependent upon voluntary co-operation for its achievements.

Research and Development, Economic Growth, and National Security*

By HAROLD J. BARNETT

ABSTRACT: Vital interest exists today in government research and development. Thinking people will discuss the underlying causes for the new interest. They may question, as Dr. Barnett does, the prevalent belief that research and development can be equated with economic transformation or that an increase in economic growth necessitates only an increase in government-sponsored research and development efforts. Accepting the fact that a correlation exists between research and development and economic growth, it is necessary to examine the paths open to the government in selecting its policy. Does not the uncertainty involved in research for future military innovations demand a recognition that multiple paths must be tried to achieve the desired results? The ends may best be achieved if government policy indicates only the objectives and allows the decisions for research-in-particular to be made by the separate research organizations. In the nonmilitary fields of research and development, it may be useful for the government to allocate funds to the universities for their research candidates and for the equipment to be used. Though research and development efforts do not necessarily cause economic transformation, there have been some demonstrable positive results, such as the revision of the negative premises for economic growth of natural scarcity theory.—Ed.

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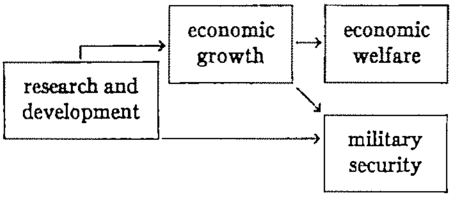
* I am indebted, for critical comments on an earlier draft, to P Cootner, K Dofis, P. Glick, W. Hirsch, C. Hitch, H Jarrett, S Miller, G. Perazich, and N Wengert. The errors which remain are mine.

SINCE World War II, there has been a strong surge of public interest in research and development. There is wide belief that government should undertake and stimulate larger efforts. The new interests and attitudes, however, do not arise from new findings and deeper understanding of the social effects of research and development. Rather, the activism has developed out of yearnings and fears—a society's aspirations are always largely compounded from emotional drives. These may, in this case, be exerting a most desirable social thrust. But the origins of the beliefs and explicit forms of social improvement which they contemplate should be examined.

Recent Interest in Research and Development

One origin of the wide interest in acceleration of research and development efforts is simply excitement over recent developments in pure science and the spurt of applied technical advances of the past fifteen years or so. Great advances in the pure science of chemistry and physics, and in atomic energy, computers, automation, satellites, plastics and other synthetics, miracle drugs, and so on, have whetted our appetites for their fruits, for more advances, and for more excitement.

The other two origins of the awakened interest in research and development are based on more reasoned premises. They are beliefs that research and development play a unique role in economic welfare and in national military security. The causation, which we now briefly discuss, is indicated in the following diagram.



It is generally asserted—we inquire into its truth later—that the rate of economic growth depends upon the pace of research and development. In turn, economic welfare is thought to depend upon economic growth. In one respect, this latter proposition is necessarily and obviously true, that is, if economic welfare is defined as output per capita and economic growth is defined as the rate of increase in output per capita. In addition, however, there is a more complicated hypothesis of the dependence of economic welfare upon economic growth. Since the thirties, very large powers for governmental intervention have been legislated into permanent United States statutes to prevent cyclical recession and depression. For the discharge of this responsibility, there is a substantial body of opinion which believes that rapid economic growth must be the major weapon. Since the rate of economic growth is believed to depend upon the rate of research and development, it is held that research and development should play a major role in promoting economic welfare, through averting depression.

Industry and ideology compete

Let us now turn to the chain which runs "research and development fosters economic growth which fosters military security":

(1) Soviet industry is reported to be growing at two or three times the United States 3 to 4 per cent yearly rate. If sustained, such difference in growth rate would have the Soviet Union surpass the United States industrial size in a generation or less. If, as is widely believed, industrial size and military strength may be equated, then the laggard relative rate of growth of the United States renders us militarily insecure, and the rate should be stimulated.[1]

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[1] For a view that industrial size and military strength should *not* be equated in view of changed military technology, see H J Barnett,

(2) The United States philosophy of political and economic freedom is in a serious cold war competition with Soviet type communism in the underdeveloped nations of the world, particularly in the Far and Middle East. In these nations, political and economic freedom are not firmly established; and democratic and communist parties must contend for popular support. The ability of Western-oriented democratic parties to retain leadership depends upon achieving rapid advance in economic welfare; the advance is necessary to meet the demands of impoverished masses seeking betterment. The view is held that if the United States can achieve sustained and rapid growth, guide these growth-aspiring nations, and provide economic assistance, then extension of the Soviet dominion can perhaps be halted. If, on the other hand, the growth rates of the democratic nations fail to satisfy aspirations and fall short of demonstrated growth rates in Russia, China, and other communist countries, then, it is feared, most of the underdeveloped nations will fall into communist party hands. In summary, the cold war is viewed as a complicated contest in economic growth between the United States and the Soviet spheres, with vast populations in Asia and Africa at stake, and wherein the United States plays the principal role as example and leader in the free world.[2]

Arms race

Finally, a major reason for heightened public concern for research and development is the recent, rapid Soviet advance in military technology. The Soviet Union has almost caught up to the United States in nuclear weapons, she has surpassed us in missiles and rockets; we face an arms race in a modern form, a race for military research and development. Indeed, in the present fluid state of military technology, there is considerable danger that the nuclear stalemate—"the balance of terror"—will be broken if we do not at least overtake Russia's missile technology lead, and match the technological pace in other major arms. These considerations indicate an apparent need for more and more effective research and development in the military sphere.

We may summarize the recent increased interest in research and development in the following way. There is great concern on the part of our government and leading citizens over the economic growth required: to maintain the industrial capacity necessary for the national security; to exert United States leadership in the free world; and to promote domestic economic welfare. If, in addition, research and development are considered major determinants of growth, then much of the interest in growth shifts to research and development. Thus research and development, already foci for reasons of military technology and scientific excitement, also become poles for the concern over economic growth.[3] For these reasons research and development have leaped into social prominence since World War II, after having been relatively ignored in public policy, with the exception of the specialized patent discussions and agricultural research programs.

RESEARCH AND DEVELOPMENT EXPENDITURES AND THE NATIONAL ECONOMY

The upper line of Figure 1 shows "real" national output—gross national product in constant prices, that is, ad-

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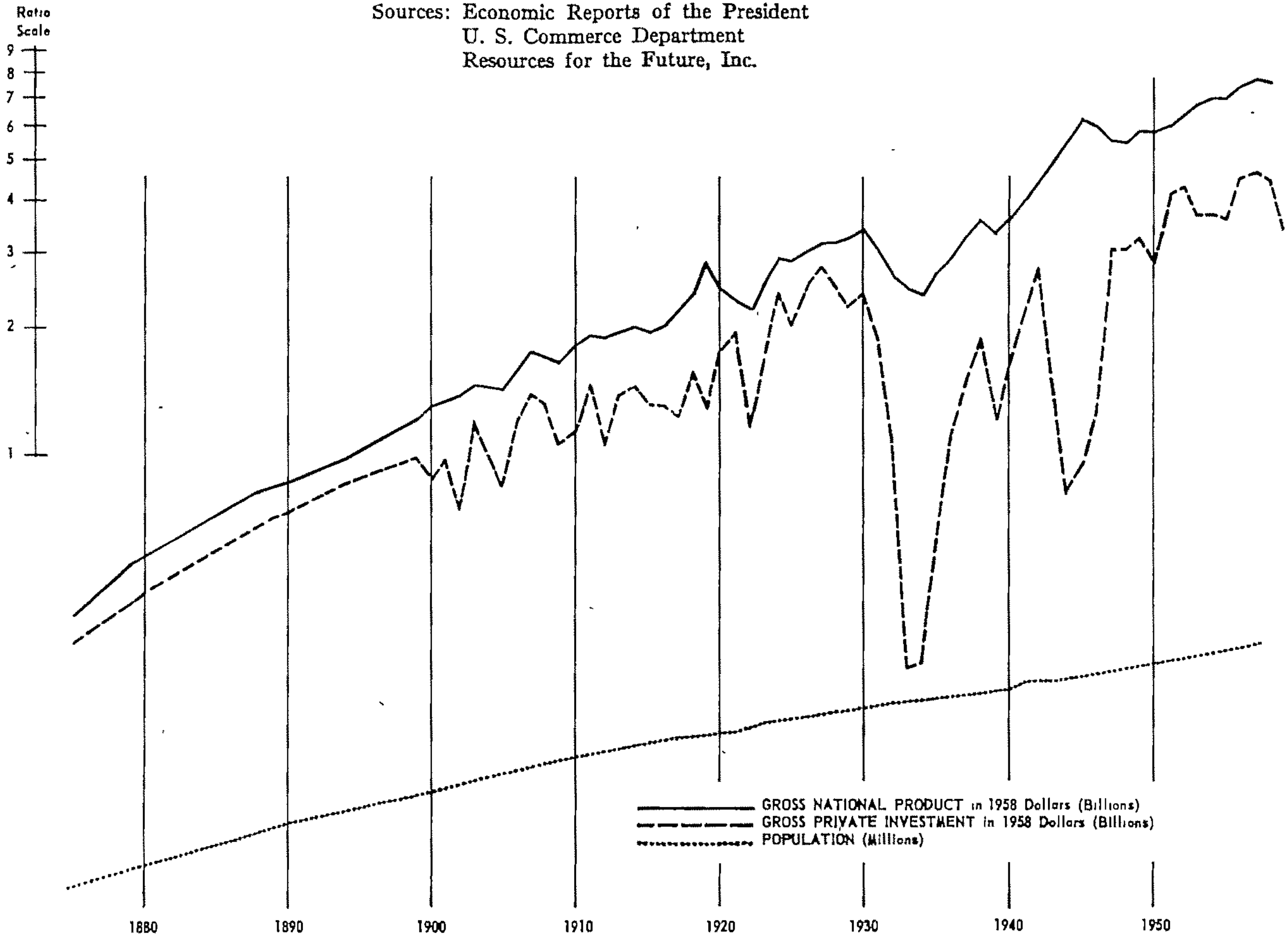
"The Changing Relation of Natural Resources and National Security," *Economic Geography* (1957).

[2] Under Secretary of State C Douglas Dillon has stated both relations of economic growth and national security, stressing the latter, in a speech "The Challenge of Soviet Economic Expansion," Washington Evening *Star* (May 7, 1959).

[3] "The significant part played by research and development activities in impelling growth as well as improving our defense capabilities is now generally recognized" *1959 Joint Economic Report,* 86th Congress, Senate Report, No 98, p 11.



Sources: Economic Reports of the President
U. S. Commerce Department
Resources for the Future, Inc.

FIG 1 Gross national product, gross private investment, and population.

justed for price level changes—and its growth since 1870. The long term growth rate is approximately 3½ per cent per year. There are also shown some of the determinants of the growth rate—population; and investment in producer durables and construction. Figure 2 compares gross national product, gross private investment, and expenditures on research and development as defined and reported by the National Science Foundation; these data are not adjusted for price changes.

The research and development expenditure figures were 1.5 per cent, 2.2 per cent, and 2.3 per cent of the national output in 1953, 1956, and 1958, respectively. These are small ratios. Research and development expenditures in 1956 were about the same as aggregate outlays on tobacco products, toilet articles and barbershops, and the same as advertising outlays; they were only 30 per cent of automobile and automobile service costs and about 60 per cent of recreation outlays.

Or, to further show that annual research and development expenditures are relatively insignificant in size in the United States economy, we note that they are only one-third as large as the loss of output which the nation experienced from excessive unemployment in the 1957–58 recession. On the average annual research and development expenditures cost the American economy only about $200 per family. This total outlay is less than the average yearly increase in family income. The impact of doubling or trebling research and development expenditures therefore, even if no benefits were received therefrom, would in magnitude involve no more than deferring income advances for a year or two.

Now if the outlays are of such small size, how does one arrive at the prevailing popular belief that the rate of expenditures on research and development plays so compelling a role in economic growth and should be increased? I can only surmise that the general reasoning might go about as follows:

> The rate of increase in real national output has been roughly two and a half times the rate of increase in labor input. Real output has increased perhaps twice as fast as the sum of labor and capital input, measured in constant prices. Then, half of economic growth is due to improved productivity of labor and capital. That is, half of economic growth is due to economic transformation. The origin of improved productivity or economic transformation is improved knowledge, which is the result of research and development. Thus, research and development can be inferred to have played a major role in economic growth. Therefore, if we increase research and development expenditures, we can hasten economic growth.

To the extent this reasoning underlies the popular belief, it permits detection of possible errors.

The major error is that which equates economic transformation with the results of research and development expenditures. In the above statement economic transformation is a residual, a catchall, for all causes of increase in the ratio of output to the labor plus capital input. Economic transformation thus includes economies of scale; social innovation of all kinds; increased quality of a literate and educated populace; shifts in composition of economic output away from low productivity sectors to high productivity ones; spatial and geographic shifts of population and economic activity; the effects of mineral exploration and findings; vocational training of labor force entrants in schools; factor (input) substitutions in the most comprehensive sense. It includes the effects of Colonel Drake's oil well discovery; of Henry Ford's tin lizzie; of Einstein's off-duty scribbling while a patent clerk; of railroad installation of diesels twenty years after their

Sources Economic Reports of the President
U. S Commerce Department
National Science Foundation

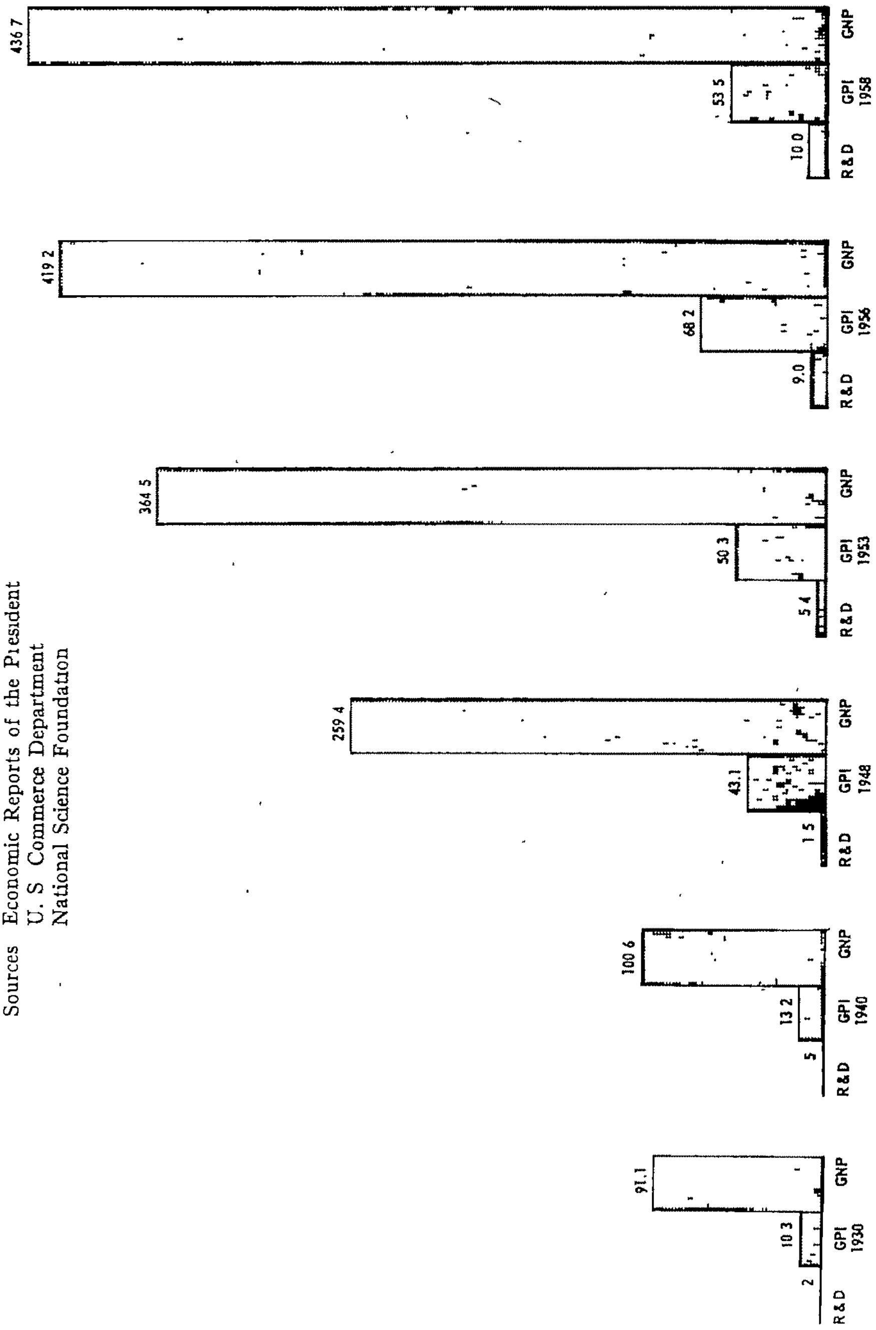


FIG. 2. Gross national product, gross private investment, research and development, in billions of dollars.

invention, financed from profits of the war and postwar years; of a great increase in international trade; and of a substantial substitution of capital for labor. It is an error to equate economic transformation, which by definition "does" account for perhaps half of our growth, with knowledge, however broadly this might be defined and even though this is one source or cause of economic transformation. Increased knowledge is not a sufficient condition for such transformation.

Errors in definition

Further error occurs if "knowledge" is equated too readily with research and development results. The major activity which perpetuates and extends knowledge and creates knowledgeable people is education, not research and development. In the year ended June 30, 1958, we produced 9,000 Ph.D.'s and other nonmedicine doctors, some of whom presumably made original research contributions in their dissertations. We also produced 365,000 bachelor's and first professional—including medicine—degrees; and 65,000 second level, master's, degrees. We educated tens of millions of school children. If we are seeking understanding of knowledge as an origin of economic transformation, should we not add education to research and development?[4] If we do so, if we have to add the nation's annual education bill of $15 billion to the $10 billion of research and development expenditures, then the cost of originating economic transformation becomes much greater. It is then more difficult to contemplate decisions which would require very large percentage increases in these outlays.

It is a further error to equate a broad definition of research and development with the much narrower concept which the National Science Foundation, for practical reasons of measurement difficulty, has had to use in its survey data of research and development expenditures. These surveys are of organizations formally engaged in research and development; wherein groups and efforts could be classified as to their purpose or aim. They are also surveys of input, not of results. But the broad definition of research and development which relates to economic transformation is one which captures not input efforts, but rather results, however informal the effort and however accidental or by-product the results are from other activities. Until it has been shown that national research and development results correspond very closely with national research and development expenditure as defined by National Science Foundation, it is a matter of faith to assume very close correspondence, and it is naiveté to assume an identity.

Finally, the very notion of a single factor cause of economic transformation —research and development expenditures, knowledge, or any other—is quite simpleminded. No one condition can ever be sufficient in a complex, dynamic economy. If we transposed all United States technical libraries to Uganda, how much economic transformation would necessarily result? For economic effect, knowledge needs capital investment to serve as carrier of the techniques; trained manpower as operators; entrepreneurs or other agents as innovators; incentives for the various parties, including, in a free economy, buyers for the goods, and profits for the risktaker; economically adequate legal and social institutions; and so forth.

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[4] Since writing this, I find that Professor T. W. Schultz has attempted to relate economic transformation to education in a more ambitious effort. For this interesting analysis, see "The Emerging Economic Scene and Its Relation to High-School Education," *The High School In a New Era,* edited by Francis Chase and Harold Anderson (Chicago: University of Chicago Press, 1957).

We now have, in our kit of technical knowledge, tools to generate mammoth increases in output, but have not the social desire, drive, understanding, or social capacity to do differently. An economy is an extremely complex social organization, a fact which the technocrats of a generation ago never seemed quite able to grasp. Why should it then be thought that further increments of technical knowledge necessarily would or should produce economic effect?

Of course it is true that availability of a large fund of technical knowledge, at home or which can be borrowed abroad, permits more rapid economic development than if the fund is small, provided that other conditions for economic change are present. The Chinese annual growth in heavy industry of about 20 per cent in its first five year plan and the reported Soviet industrial growth of 10 per cent last year demonstrate this, if the reported figures are true. But technical knowledge alone is not enough; social drive and other economic conditions are also necessary.

PUBLIC POLICY AND ECONOMIC MANAGEMENT

Most of us subscribe to the belief of our political and military leaders that Soviet expansionism and adventurism are likely to precipitate world war if the United States does not at least match the pace of Soviet weapon development and strength. If the view is valid as a basis for policy, then the probable cost of United States failure in this regard is at best vast destruction and conquest of the free world by the Soviet Union, and at worst something approaching world suicide. Preservation of the stalemate by matching military weapon development during a period long enough to permit reason to become dominant in international relations is, I assume, a prime national objective.

Military research and development

If we have such an objective, then there is a corollary for military research and development behavior. We should, I think, act in spite of our substantial ignorance concerning correspondence of results with research and development input efforts. If we observe Russia outpacing us, as is true today, I see no alternative to thrusting greater funds and manpower into the military research and development effort in the hope that results will at least move somewhat in the same direction as effort. If we act in this manner, we may be committing error; it is possible that the combination of expansion in this sphere and our lack of management skill may, particularly in the short run, produce just the opposite of the result intended. This has happened before in economic affairs. But the greater probability is that the increased effort will help, not hurt, both the pace of military research and development results and the pace of improved weapon procurement. This tends to be the greater probability because in individual narrow spheres—a company, an industry, in this case military hardware—we have been able to observe substantial correspondence between research and development results and research and development efforts. The reason it tends to be more true in individual spheres than in the economy as a whole is because other economic sectors can be robbed of their talent, and because the objective is not so ambitious as generalized economic transformation of a free society.

But with respect to programming and management of the military research and development effort, the evidence is piling up that uncertainty of the outcome, of specific results, is the most basic characteristic of military research and development The corollaries to this finding, which has analogies in a great variety of other areas characterized by high uncertainty—from raising children to the economic principles appropriate for a free choice society—are:

> 1 to confine high level programming and direction to statements of objectives, leaving to research personnel the choice of pathways and techniques for achieving them;
> 2. to conduct research and development in discreet sequential stages, in order to "buy" information and remedy ignorance, so that expensive final development and procurement decisions will, to a maximum degree, rest on valid, knowledgeable premises;
> 3. to foster substantial competition and "duplication" in research and early prototype efforts—where the uncertainty and unpredictability are greatest—both to buy information and to spur inventive genius.

There is a great deal of direct evidence that centralization and strong, detailed, and early programming of military research and development tend to be less productive of major advances and more costly than more competitive, uncontrolled, and exploratory approaches. Two exemplary cases are the World War II Manhattan District and Office of Scientific Research and Development. The former sponsored vigorous competition among different groups working separately toward the same objective, with great success. The latter could not have been more un-co-ordinated and still be called an organization. Yet it was also spectacularly successful.[5] For the postwar period the example of missiles, in which the United States is still not out of the crisis stage of having fallen behind the Russians, is instructive. We made the mistake of deciding early that long range ballistic missiles had prospects too poor to justify major research and development efforts. And then, when the Russians proved this wrong and we started to rectify the error, we almost made the mistake of too early decision as to which missile research and development efforts should be pushed. Fortunately, interservice rivalry kept competition alive sufficiently long to prove its practical necessity in the uncertain research and development sphere of ballistic missiles. Burton Klein's comment on this point is informative:

> Ten years ago the experts generally agreed that ballistic missiles were not likely to be nearly as promising bets as long-range cruise missiles Things went wrong when the government, on the basis of these shaky predictions, chose to spend fortunes —and large fortunes they were—in the development of cruise missiles and to spend comparatively negligible amounts on ballistic missile technology . . . .
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> A recent instance, and one that shows the value of having independent agencies sponsor research and development, is the Navy's development of a solid-fuel rocket engine. Several years ago none of the services was much interested in such an engine. But when the Navy decided to develop a submarine-launched ballistic missile capability, the Polaris, it had to develop a solid-fuel engine. A liquid-fuel engine just wouldn't do. As a result it found that this was a surprisingly promising engine—so promising that now all three services plan to use solid-fuel engines[6]

Multiple paths

C. J. Hitch, head of the RAND Corporation's economics division, has tried to indicate how the competitive principle can be brought to bear in military research and development. He points out that the most expert predictions of the results of research and development efforts have been notoriously unreliable. Frequently the only way to learn whether an idea will work is to test it; therefore, the quickest, and frequently cheapest, device is to try multiple paths The greater the importance of the problem and the greater the uncertainty, the more is such "wasteful" behavior required. Also, the cheaper the multiple paths, the more sensible to try, and they are cheaper at the early stages of the research-development-procurement chain. Finally, the competitive spur among people and groups is a most effective one, a fact the Russians have also learned and successfully exploited in research and development.[7]

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[5] See C. J. Hitch, *op.cit.*, below.

[6] B. Klein, *What's Wrong With Military R &D?*, RAND Corporation, P-1267, pp 15–16 and 21.

B. H. Klein's and W. Meckling's other RAND case studies of past research and development results also emphasize the inherent uncertainty and the error potential of too precise and too early programming. They point out that of twenty-four aircraft developed since World War II, in only three was the final engine the one initially planned. Some engines were designed for fighters, others for bombers, but original intent had little influence on ultimate use. One engine, originally designed for a bomber, was so successful it was used in eight fighters and five other bombers With respect to missile uncertainty, Klein and Meckling paraphrase a *Time* magazine report as follows:

> The ICBM program suffered from a lack of support because the guidance problems were so severe that the rest of the program was not pushed. The unexpected development of the H-bomb suddenly made even very inaccurate ICBM's useful. We were unfortunately at that time in no position to benefit immediately from this development. We would have been in an even worse position but, luckily, an entirely different program—the rocket booster for the Navaho cruise type missile—had been pushed so far that we could use it as a basis for the ICBM engine.[8]

Three main points stand out from the evidence. First, the type and degree of programming should be related to the degree of ignorance and uncertainty. In early stages of exploration—compare the Lewis and Clark expedition—it is frequently the greatest wisdom merely to point the explorer in the desired direction and state a general goal. Second, research should be viewed as a dynamic process because even unsuccessful efforts reduce ignorance. The new information should be used in making revised decisions of goals, directions, and ultimately, pathways. Third, the competitive drive and process are powerful instruments to research accomplishment; they can be robbed of potency by bureaucratic constriction.

Nonmilitary research and development

Outside the military area, we have a major concern for sensible government research and development policies in the interest of economic welfare and growth. Both welfare and growth are new statutory social objectives of the federal government—in the Employment Act of 1946, as amended, and elsewhere—and so there is no question of the propriety of government activity, even on a large scale.

But there is a question of what is sensible behavior, in view of our great ignorance of the social relation of government research and development to national welfare and growth. I think it follows from our ignorance that our first priority should be economic and other social research on the subject of research and development. We should become very active in sponsoring study designed to learn and understand how significant additions to technical knowledge are made, what are their economic and other social effects, and what these analyses suggest for government research and development behavior. Until we know more, there is a danger in being overly active in government-sponsored research and development.

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[7] C. J. Hitch, "Character of Research and Development in a Competitive Economy," *Proceedings of a Conference on Research and Development and Its Impact on the Economy,* National Science Foundation (Washington: U. S. Government Printing Office, 1958). An early seminal and general statement of the danger of too detailed or too distant programming under conditions of uncertainty appeared in an internal RAND paper by A Alchian. The Committee for Economic Development has recently come to similar conclusions—see *The Problem of National Security* (July 1958), cited in *Proceedings of a Conference on Research and Development and Its Impact on the Economy, op. cit.,* p 14.

[8] B Klein, and W. Meckling, "Application of Operations Research to Development Decisions," RAND Corporation, P-1054, pp. 13–16.

While in general we should be initially quite passive with respect to government stimulation of nonmilitary research and development, there are some probable exceptions to the generalization.

One possible exception is that of government grants to higher-level educational institutions, without specification as to purpose beyond their use in university education and research. This would be more an act of faith than a reasoned step to economic welfare and growth. But it has several virtues over the more purposive device of trying to improve welfare and growth through technical research and development programmed in its particulars by the government. First, our belief that economic progress derives from higher education —including the research and development that faculty and graduate students do of their own choice—is stronger than that such progress results from programmed technical research and development. Second, general grants of the type suggested avoid the danger of distorting the production of Ph.D.'s and other graduate students, and of malforming or unbalancing the accumulation of understanding and knowledge. For example, most of our intellectual leaders believe that the limiting factors in growth and welfare are more to be found in inadequate economic and other social knowledge and understanding than in physical science areas. This is why, for example, outstanding technical schools like Massachusetts Institute of Technology, Carnegie Institute of Technology, and California Institute of Technology are now giving increased emphasis to the social and behavioral sciences. I would suggest that the objective of advance in knowledge can, both with respect to the areas selected for emphasis and with respect to the forms of activity necessary for knowledge acquisition, be better managed by the universities, traditionally responsible for this function, than by government legislators and administrators.

Research equipment allocation

Another possible exception to the generalization that government, pending improved understanding and skill, should be extremely passive in economic allocation to nonmilitary research and development concerns research tools. If, somehow, the government could sponsor improvement in the quality, and perhaps quantity, of research equipment and information available to be drawn upon by independent research efforts, then government would be improving productivity within research and development, without, however, risking distortion or misdirection of research and development efforts. In part this is already an active government function: library and information dissemination work, translations, research on and production of radioactive tracers, research on computers, standards and measurement research, geological exploration, and so forth. The cases in which tool improvements—such as tracers and computers—permit research never before possible are very exciting.[9] But the question is whether such efforts—making available tools and information, and training in the use of new tools—should be expanded. The danger is that in some of these functions the government inevitably implicitly favors certain areas and prejudices others. And what starts out to be a neutral effort to increase productivity in research and development could turn out to be economically very "unneutral," even apart from the direct influence of political pressure groups.[10]

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[9] Re tracers, see H. J. Barnett, *Atomic Energy in the United States Economy*, Ph.D. dissertation, Harvard University (August 1952), Chapter 2.

Other areas for economic research

A long-standing problem concerning technological change is its possible tendency to create unemployment, at least in the short run. This obviously deserves study. For example, it is clear that distress among farmers is to some extent attributable to rapid technological change in agriculture, deriving in part from government-fostered research and development. Another important area for economic research is the role of research and development in making the economy effectively competitive. Research and development results can play a major role in the creation of "countervailing power"—that is, in developing substitutes for commodities subject to private market control. On the other hand, there are clearly cases in which research and development have, at times, operated significantly to reduce competition—the aluminum and shoe machinery cases are examples.

There are many other areas which need economic research before we may with assurance become active in artificially stimulating research and development. For example, it has been alleged that research and development play a most important role in growth and full employment by increasing the demand for consumer and capital goods; that the research and development sphere operates under increasing returns to scale, since each new piece of knowledge can be connected with all other preceding pieces; that research and development activity promotes economic stability, since it is not subject to cyclical recessions; and that there is no necessary relation between the industry of research discovery and the industry of application of research results. Some of these are discussed in the recent National Science Foundation volume, particularly in the articles by Professors Slichter and Fox.[11]

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[10] I am dissatisfied with my discussion in the four preceding paragraphs First, as noted, it is rather subjective. And, second, the implicit premise of optimality in the structure and processes of research and education is suspect Thus, I am uncomfortable about some of the questions which my critics have asked. For example, Professor Norman Wengert has asked, "Are you assuming an unseen hand operating in the choice of Ph.D. major fields and dissertation subjects? The idea of knowledge for its own sake is appealing, but in fact many pressures are at work, some of which are completely irrelevant and irrational in pushing graduate students in one or another direction" And, at another place he points out, "The universities, of course, also program their activities, and except for the top institutions, I am not at all sure they do as good a job as some federal agencies. It is my feeling that the manner in which our agricultural research has developed, especially that undertaken by the land grant colleges, represents a desirable kind of programming, because it is so largely permissive, resting on agreement as to general areas of investigation but leaving specific implementation to the several research stations and their personnel. The role of pressure groups in agricultural research, of course, poses a special kind of problem." Professor Werner Hirsch has raised a different embarrassing question, "I am greatly intrigued by the problems that arise when government, and the same holds for foundations, steps in and finances research and development activities Whose work should be financed? If we agree that we do not know in advance which is the most promising area of research, what guide lines do we have to allocate funds? In the past we have given money to those who have the best reputations and are associated with the institutions having the best reputations. In doing so we run a most serious risk of weakening our research system and our system of higher education. A few institutions have bushels of research and development funds. They actively recruit researchers from middle-sized and small institutions; particularly the better people are induced to leave the second-echelon institutions. If this continues, we will end up with relatively few excellent institutions and masses of very poor ones, obviously a very narrow base on which to build research and development in the long run."

RESEARCH AND DEVELOPMENT VERSUS NATURAL SCARCITY

I close with a few affirmative paragraphs on an exciting economic and social accomplishment of research and development, the gradual erosion of a basic premise of "the dismal science"—a premise which was once believed so certain it was viewed as natural law. I refer to the accomplishments of research and development in undermining the economic significance of natural resource scarcity, both in doctrine and in fact.

The Malthusian dilemma was that population would grow rapidly, and that the pressure of population on limited agricultural land would inflict diminishing average and marginal returns to future increments of population. Ricardo's subsequent innovation indicated that natural resource scarcity obstructs growth long before the Malthusian limits are reached, because incremental unused land is of persistently declining quality. Jevons transposed the Ricardian position to coal availability, which he viewed as a sine qua non of economic welfare; and he introduced another force responsible for natural resource scarcity, that is, depletion. The American conservation movement extended the doctrine of natural resource scarcity as an obstacle to economic growth. It found the natural resource limits to be multidimensional; and, since natural resources are a system of interdependent variables in ecological balance, then over-all scarcity can be engendered by quantity or quality limits of a single resource.

These and other warnings of strenuous conflict between natural resource scarcity and economic growth were not trivial expressions. Yet, with hindsight, we may observe that their authors, some of them great thinkers, committed a major error, and so their prophecies, at least for our time and the foreseeable future, are wrong. They neglected or gave inadequate attention to the results of future research and development and of other increments of knowledge. What the brilliant scientist, Sir William Crookes, did not foresee when he predicted only starvation for growing populations from diminishing supplies of nitrogen, was that fixation of nitrogen from the air would be discovered a generation or so later and remove this threat of disaster. Prophecies of crises from exhaustion of favorable coal reserves have similarly failed, due to the inability to foresee other research and development results—the opening of alternative energy sources and advances in coal mining technology. Gifford Pinchot's forecast of economic blight from timber famine has failed because he did not foresee a further research and development result—technological substitutions sufficient to make the economic effect of reduced timber supplies and higher timber costs of very minor national economic importance.

In agriculture, research and development have facilitated the economy's transition to engine and electric power and relieved agricultural land of the burden of providing calories for work animals. If the United States today had to rely upon work animals for its horsepower, the feed for these animals would require fifteen to thirty times as many acres of crop land as are in use in the United States. Land shortage has been further averted by more recent research and development which have made possible the production of synthetic fibers, releasing natural fibers equivalent to five to ten million acres. Recent developments in chemical fertilizers, insecticides, hybrid plants, and so on, have greatly increased crop yields per acre.

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[11] S. H. Slichter, "Technological Research as Related to the Growth and Stability of the Economy," and B Fox, a discussion of an article by R. E Burgess, "Impact of Research and Development in the Chemical Industry," *Proceedings of a Conference on Research and Development and Its Impact on the Economy*, National Science Foundation (Washington: U S Government Printing Office, 1958).

In minerals, natural resource shortage, in the face of vast increases in mineral demand—more than thirty-fold since 1870 and four-fold since 1900—has been averted by a combination of two means. One is not research and development in a narrow sense—the finding of new reserves as fast as known ones are used. But the other does rest on research and development—improved methods of production, extraction, transportation, refining, and utilization, which have offset the fact that the new reserves are frequently of lesser physical grade or less accessible. Further, baneful economic effects of incipient shortages in individual minerals have been averted by research and development of inexpensive substitutes based upon economically more plentiful resources—aluminum, taconite, magnesium, and plastics are examples.

In modern economies, the meaning of goods and resources has changed radically because of research and development. For example, more than 90 per cent of the increase in real gross national product in the United States since 1870 has been of nonagricultural origin. The method of transforming materials into final goods has become far less merely a mechanical one; rather it is now, to a considerable degree, a controlled heat and electro-chemical process. Finally, the natural resource building blocks have changed very radically; they are now atoms and molecules. This has changed the meaning of natural resources for societies which indeed have access to modern technology and capital. The same ultimate world physical limits still exist, of course, but not the same economic limits. How many taconite iron atoms and sea-water magnesium atoms and bromine molecules constitute plentitude, and how many scarcity? Furthermore, recent research and development have given us a three-dimensional world, as compared with the one which was economically sustained almost entirely by surface resources.

Of course, research and development have not vanquished once and for all the threat of natural resource economic scarcity. Indeed they can never do so with certainty, so long as populations continue to increase—the galaxy itself is not large enough to sustain infinite demand. This is what accounts for considerable apprehension among some students of natural resources and population growth. But I would point out that we have come very far from Malthus, yet have only penetrated a tube four miles into the earth's crust, still have not mined the ocean floor or the seas themselves, still have vast areas of the globe which have never been seen by man. We should not underrate the further results of research and development—assuming we avoid world destruction while we employ the most recent achievements.[12]

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[12] For bibliographical references and more extended discussion of natural resource scarcity and its relation to economic growth, see S V Ciriacy-Wantrup, *Resource Conservation* (Berkeley and Los Angeles. University of California Press, 1952); A. Scott, *Natural Resources: The Economics of Conservation* (Toronto, Ont . University of Toronto Press, 1955); H J Barnett, *Malthusianism and Conservation* (Resources for the Future, Inc, 1958); and H. J. Barnett, *Measurement of Natural Resource Scarcity,* to be published by Princeton University Press in a National Bureau of Economic Research volume on Income and Wealth.

Science, Government, and Society

By Vincent Heath Whitney

ABSTRACT: Historically men have sought knowledge in a variety of ways both for its own sake and in order to increase their control over the environment. Science has increasingly made its impact on society through technical applications of scientific findings. Science has thus helped to alter social structure. At the same time increases in population, in social differentiation, and heterogeneity have made necessary large-scale organization. The effect has been felt in the structure of science and the role and status of the scientist; for example, the individual scholar has been replaced to a considerable extent by the scientific organization man. Government encouragement of some areas of scientific research has expanded enormously in the United States, inevitably within a bureaucratic framework. In the process the scientist has come to be valued not for his scholarship but for his ability to provide solutions to specific problems and thus to increase national power. Bureaucracy has a price, but it is an organizational form which is understandably and functionally necessary and to which science must adapt.

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EVERY article in this issue of THE ANNALS illustrates the relationships among science, government, and society. The majority deal with specific consequences of the expansion of scientific activities by American federal, state, or local governments. As a result, I do not propose to repeat here the specific history of scientific development in the United States, especially of the use of science and scientists in government, nor to re-examine the particular illustrations of the consequences of such development for the economic system, the universities, the field of public administration, and the many other concrete areas which have already been presented in some detail. Instead I shall try to indicate in general terms and from the perspective of sociology the significance and some of the possible consequences of the recent rapid expansion of scientific activity in and out of government.

THE PLACE OF SCIENCE IN SOCIETY

The search for understanding is as old as the history of mankind. Understanding has been sought for two related but distinct reasons. The first is to satisfy the desire to know for the satisfaction of knowing, and the goal is "pure" knowledge. The second is to gain control over some aspect of the material or human environment, and the goal is power. Each era has had its predominant method of seeking understanding. In small preliterate societies trial and error or magic has served as the approved way to search for new knowledge and control. Philosophy, religion, and science are other ways of formulating problems and seeking answers which have come to characterize the intellectual activities of specific times and places. Probably no one approach has ever had exclusive ascendance; and this is especially true in modern civilizations which are characterized by a high degree of differentiation and variation. But even here some dominant method of seeking the answers to intellectual problems can be identified for each society. This method is a reflection of the prevailing organization of human activities in that society and consequently is also a function of the accumulation of prior knowledge and understanding at any given historical moment.

Stages of scientific inquiry

This suggests why—although the tradition of science is one of long standing[1]—the utilization of science as the characteristic method of inquiry in any society is so recent, if indeed it yet stands clearly above alternative approaches. Certainly not until the nineteenth century did science achieve wide acceptance anywhere. Earlier, scientific inquiry had been the exclusive property of a tiny group of persons, generally men of unusual education, leisure, and wealth, who were uniquely able to pursue knowledge for its own sake. For these private enterprisers science was not a profession nor an occupation. Neither was it a way to find solutions of immediate practicality to current technical or social problems. It was rather a predominantly intellectual activity, pursued in a spirit of disinterested inquiry, and so it remained until very recent times. Ultimately the findings of these independent scientists, mostly through the applications made by other men, were to alter social structures and related individual attitudes and behavior profoundly. For the moment the knowledge that resulted from their patient searching remained the property of a fragment of the population and served the important function of raising new questions which demanded still further search for answers. It did not directly affect the lives of hunters and fishermen, peasants, serfs and nobles, guildsmen and merchants, nor did it alter the organization of families, economies, religions, or governments. In short, it was not an activity affected with an apparent public interest.

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[1] See George Sarton, *Introduction to the History of Science* (Baltimore: Williams and Wilkins Co., 1927). Volume 1 uses nearly 800 pages to cover the period from the time of Homer to that of Omar Khayyam, that is, to the second half of the eleventh century.

The impact of science gradually came to be felt by a wider segment of society through the applications of scientifically derived knowledge. In those nations which were undergoing the series of technological innovations and social changes customarily called the Industrial Revolution, individuals who were not themselves scientists were able to utilize scientific findings to create wealth. Their medium was the technical application, and their interest in science was its use to increase their control over some aspects of their environment. Ownership, or even use, of such applications was the basis for the rise of a new business élite and was so clearly associated with improvement of social status and economic and political power that science and technology came into an inseparable association. Science was increasingly seen as useful and practical to the extent that it promoted productivity and higher standards of living. Since these were valued positively in nations undergoing modernization, it is not surprising that the scientist was increasingly in demand to provide specific kinds of knowledge which could solve technological problems or improve technical processes of production. As a consequence the scientist as a learned scholar working in relative isolation on problems of no immediate practical worth was rather rapidly replaced by the scientist as a member of an organized profession whose work was dictated by problems requiring immediate, concrete answers. Inevitably the demand for scientists rose sharply with this new estimation of their practical worth. Scientists came less and less to work alone, more and more to form a part of a team associated most frequently with industry and with certain private organizations, particularly the universities. Although the latter were sometimes viewed as the last refuge of the scientist as true scholar, such a picture was itself imaginary. The university scientist, as much as his colleagues in industry, found organization necessary both because of the nature of the problems on which he was now working and in order to win the support of industry. Moreover, a high proportion of the students who filled the classes of the university scientists were being trained for positions in industry.

Science and society

Science was one factor producing pronounced alterations in the social structure. It was inevitable that the new organization of society would similarly alter the role and status of the scientist and the position and character of science itself. The leading sociological theorists, from Auguste Comte to Max Weber and Karl Mannheim, have always emphasized this close relationship between the social structure and the personality and behavior of the individual. One cannot be understood apart from the other.[2]

To explain the alterations in the position of science in the United States and the changes in the role, attitudes, and behavior of the scientist it is essential to explain the parallel changes which have occurred in the several institutional systems, such as work, family, religion, and government. This is because of the interdependence of persons, families, and communities—and, in a broader context, of nations—and the multiple linkages among social institutions. Obviously, not every person influences every other person, even indirectly. It is equally true that not every institutional change produces a direct and immediate reaction in all other parts of the social organization. Yet there are long chains of subsequent effects set in motion by prior effects which can at least in theory be traced back to an initial change. Ultimately, innovations of sufficient importance modify an area of societal organization, such as government, and require either further modifications in that particular institution or adaptations in related institutions. This is because a society is an organized system and not a series of discrete systems without linkages. It is impossible to maintain science today as a disinterested inquiry into purely intellectual problems by lone individuals utilizing their own leisure and funds. It is as impossible as it would be to maintain the federal government as a weak agency of a federation of jealous states limited to a few hundred employees at most and restricted to a small number of clearly defined tasks such as raising a militia and defending the nation against actual invaders.

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[2] C. Wright Mills, *The Sociological Imagination* (New York· Oxford University Press, 1959), pp. 5–8.

Changes in American society

Science is a part of, and one among many causes of, the thoroughgoing transformation of society which has taken place in all those nations which have undergone modernization and, to a lesser extent, in those which have not. There is no space here to trace these enormous changes, but they are known in a general way to all of us. A few of the most important can simply be noted. First, knowledge has been vastly multiplied and is still enlarging at an increasing rate. In anthropological terms, the culture base has been extended. The longer the base, the greater are the opportunities for recombining items already in existence, which is the basis for developing new artifacts and new ideas, in short, new knowledge. Man's increasing inventiveness is a function of his ever widening understanding rather than of an increase in his innate intelligence. This increasing inventiveness produces, second, a higher rate of technological and social change than past societies have known. Paradoxically, change has become a constant—to the point where some educators insist we can train our children only to be flexible. Third, our population in the United States has increased fairly rapidly and is continuing to do so with the result that there are now nearly 180,000,000 people in the country. Fourth, despite our increasing population, we have raised even more rapidly our per capita income. This has been based on increasing specialization and differentiation of functions made possible by rises in many technological and economic factors such as utilizable innovations, capital per worker, and quantity of output,[8] and by social inventions and changes as diverse as new forms of business organization, contraception, and the increasing employment of women outside the home. Fifth, heavy immigration and high internal mobility, coupled with a high level of specialization, have produced a nation characterized by wide differences in beliefs and behavior, in status and wealth, in occupation and type of residence, in short, a heterogeneous nation.

If we attempt to summarize this great transformation of American society, we see that it represents a fundamental change in social structure based principally on discovery and invention, which have been greatly augmented as basic research and applications have both increased our knowledge and our ability to acquire additional knowledge. Closely related have been a rapid rise in population, made possible by the increase in our ability to support human life adequately, and the development and spread of metropolitan communities, in which some half of the American people are now living.

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[8] Some of these are discussed in Sumner Slichter, "Some Economic Consequences of Science," *The Annals:* Vol 249 (January 1947), pp. 99–110.

Organization displaces the individual

The United States is a big country both in area and in population. That is the explanation at the most elementary level for the fact that it has developed big businesses, big labor organizations, big government, big religious organizations, and big research organizations. Organization reflects the demonstrated fact that goals involving the separate actions of a number of persons can ordinarily be attained more effectively when each person is assigned certain duties than when each is allowed to act as he thinks best at the moment. There are exceptions to this in the actual execution of much scientific research, where the individual worker can secure the best results if he is simply given an understanding of what is sought and the freedom to pursue whatever approaches seem to him most likely to be fruitful. But even such activity ordinarily takes place within an organizational framework; and this kind of freedom is in fact part of the plan, a sign of good organization.

Large size, elaboration, and complexity themselves cost something in terms of waste of time, effort, funds, and enterprise. I am not arguing that they do not; sometimes the price is prohibitive. Neither am I insisting that in the United States today there is no place for the individual scientist working largely on his own. I am saying that in an era of large-scale organization and in human societies where institutional systems are interrelated, it is not possible for one area of activity, scientific research, to retain the characteristics which were associated with it in a society in which small-scale operation was predominant. Scientific research is a type of behavior which requires certain predisposing attitudes such as a recognition of the necessity for objective observation and the desire to report results honestly. It is a patterning of relationships among men based on understood rules which have been developed over time to support those values which scientists believe necessary if their goals are to be achieved. These attitudes and relationships undergo modification as the social structure, of which they are a small part, is altered. They have clearly been affected by the increase in size and complexity both of large segments of contemporary society and of the more limited area of scientific inquiry itself.

This means that the individual has been superseded as the customary unit of scientific investigation. He has been replaced by the organization. In physical science, and increasingly in social science, problems to be attacked are too big for one man. They require the approaches of several types of specialists, of men from several disciplines. They demand more knowledge than one man may possess or readily acquire. They cannot be answered without laboratories and computers and other types of equipment which are expensive and often not available to one investigator. I hope there will always be scientists who can reflect on grand questions on a grand scale or pursue small projects of no immediate apparent consequence without organizational help or interference. I hope that organizational structure can be greatly improved to create the freest possible effective working conditions. But these are matters to be discussed at another time. Scientific research cannot proceed on a seventeenth-century basis in the twentieth century.

SCIENTIFIC RESEARCH WITHIN GOVERNMENT

So far I have purposely avoided any reference to government interest and involvement in scientific research. Many of the points which have already been made help to account for the tremendous increase in scientific research and research training under government auspices, described in other articles. Clearly, large-scale government, despite its ineptitudes and even harassments, is the single organization big enough to cope alone with the requirements of large-scale science. The federal government today is inevitably immersed in power politics on a world scale. The steady flow of technological applications which, rightly or wrongly, is thought to be essential to the maintenance of national power and status, and in some areas to impose requirements of secrecy, makes necessary not only vast numbers of technologists with some training in science but also an ever increasing number of scientists, some of whom are presumably engaged in basic research.

There is a need for social scientists and social science research although in comparison with the demand for scientists and research in the natural sciences this need is perceived only dimly.[4] For if American society is to continue along present lines it must strive to avoid war, maintain full employment, and develop controlled use of natural resources essential to an industrial economy.[5] There is a noticeable need for basic research in the area of population—studies of fertility, population growth and distribution, and migration—since all of these profoundly affect American life. The very size of these problems again clearly indicates that they can be studied adequately only with the support of the federal government or some other large organization. Of course, state and local governments can and will be involved as well but to a much lesser degree. Similarly, government will utilize the facilities of private research institutes, colleges, and universities, principally the latter, to support particular research projects. Industry too is spending many millions of dollars each year on research, but this is understandably research for private purposes. Some of it may be of great significance ultimately for the American economy and society. But this in no way obviates the fact that research on problems of national import cannot be left to the chance interest of individuals, private organizations, or lower levels of government.

There is still a sharp inclination in the United States, strongest perhaps among laymen but also readily identifiable among scientists, to emphasize the desirability of independent individual effort and to deprecate the necessity for increasing involvement of government in all aspects of life including research activity. This is one variation on the nostalgic wish for a return to the "good old days." Such a wish when examined carefully usually turns out to be a wish to recapture certain selected attributes of a total situation which would no longer be acceptable. Many of us in our longing for grandmother's homemade ice cream forget the laborious task of turning the hand freezer; and in the last analysis we prefer to take our ice cream factory-made after all. Similarly many of us have a personal preference for the older conception of the scientist as a disinterested scholar pursuing truth for truth's sake according to his own interests, and some of us may still be able to assume such a role. By the same token we resent, and up to a point, resist, the bureaucratic structure which develops around all large and unwieldy activities. As an alternative, we accept bureaucracy and become "conformers," a term which is frequently used as an accusation or an epithet today. I am not arguing for conformity when I repeat that as increases occur in the size, differentiation, and complexity of any structure, for example science, increasing organization of activities is a functional necessity. The problem for the scientists who seek to defend such values as freedom—which I purposely leave undefined—is not how to prevent organization but rather how to shape organization to maximize their own goals and to minimize practices and procedures which they believe to be inimical to the pursuit of their activities. One way is obviously through organizations of their own.

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[4] See the article by Harry Alpert in this issue.

[5] Detailed statements will be found in Harrison Brown, *The Challenge of Man's Future* (New York· Viking, 1954) and Richard Meier, *Science and Economic Development* (New York: John Wiley and Sons, 1956).

Some consequences for society

In itself the interest of the American government in scientific research is not new. As Louis Wirth has pointed out, "Franklin and Jefferson urged it, and our Constitution provides for it. Federal, state, and local agencies have themselves for many decades carried on scientific research in many fields."[6] The National Academy of Science was created by Congress in 1863 at the request of President Lincoln. President Wilson established the National Research Council at the time of the first World War. In June 1941 an executive order of Franklin Roosevelt set up the Office of Scientific Research and Development.[7] Specific departments and agencies have sponsored research of diverse character for some time and have clearly established the principle that science is invested with a public and indeed a national interest.

But this is not to say, as Wirth does, that there is "nothing new" about scientific research activities of government and their consequences both for the scientific community and for the larger society. The vastly increased scope of government involvement is itself new and the impact of such a program is far more wide reaching than anything previously known in this country.

A fundamental consequence of government in science is the shift in emphasis both as to what constitutes the main concern of science and also as to the proprietorship of scientific findings. The scientist assumes importance in the eyes of Congress, the government administrator, and the general public not because of his erudition or his scholarly aims or his ability to add to the total of human knowledge but because he can provide practical solutions to specific problems. To use my earlier figure, the scientist can help the administrator to control the environment and thus to increase his organization's power relative to inanimate objects or other organizations of human beings, for example a foreign government.

The scientist thus becomes a unit in a more or less impersonal structure developed initially at least for some rather specific purpose. He may work with relatively great or relatively little freedom but he will be working for an organization which provides him with necessary support, including his own livelihood, and which in consequence claims for itself the right to "own" his findings, permitting them to be disbursed if it sees fit or restricting them for any of a variety of reasons. It may be because an administrator believes there are legitimate grounds for enforcing secrecy. It may be because Congress or rival agencies or other "busybodies" can less readily assess the value of information or the need for continuing specific programs when materials are classified. It may be simply a reflection of the fact that bureaucracies tend to self-aggrandizement.

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[6] Louis Wirth, "Responsibilities of Social Science," *The Annals:* Vol 249 (January 1947), p. 144.

[7] *Idem.*

Bureaucratic organization can hamper science and introduce inefficiencies and frustrations. These may be fatal for individuals but they are unlikely to hamper the advancement of science itself seriously, particularly if scientists act in a mature and responsible way to make clear to administrators the conditions under which scientific work thrives and to insist on free exchange of all possible knowledge not only beyond bureau or agency boundaries but beyond national boundaries. Though problems develop when science becomes an organized activity one must not forget the vast stimulus to the training of scientists and to their use which is provided by the great increase in government scholarships and employment, described elsewhere in this issue.

In this expansion, just as the role of the scientist changes from that of scholar or intellectual to that of organization man, changes in status will also occur. The demand for trained men is likely to increase economic status. The increase in numbers and anonymity may decrease social status and prestige for the average scientist although the exact nature of such changes, especially in the face of the scientist's already ambivalent status, is hard to predict.

Overemphases

It is entirely possible that some areas of science will be disproportionately emphasized at the expense of others. To cite two examples, government has recently seemed more willing to put funds and men into nuclear research than into acoustics; into natural science than into social science. In the latter case there is frequently a failure to realize that science not only solves problems but also creates them and that many of these are at heart problems of human relations. Controlling the use of nuclear devices demands more research in political science, sociology, and economics. Despite the requirement to do so in the Atomic Energy Act, the Atomic Energy Commission has yet to undertake research assessing the social and economic impact of nuclear development on the American economy. Other examples might be cited, but the point is that a heavy concentration of research activities in government or under government sponsorship can hinder the development of specific fields at the same time that it promotes that of others. The same thing is true if overemphasis is placed on research applications at the expense of basic research. The results can be costly if scientists working in the neglected fields fail to make themselves heard. This is especially so with respect to social science since the major consequence of the increased emphasis upon natural science in recent years has been to extend the ability of individuals to produce and to destroy material objects without a compensating extension of knowledge required to assure that society will benefit from these technical advances.

In sum, the changes which are taking place in scientific activity reflect similar changes which can be observed in other areas of American society. Here we find that there have been a great many changes in the role and status of workers associated with the proliferation of occupations and the general rise in skills and the employment of skills. Notable has been the shift from self-employment to employment within large organizations on a wage and salary basis. These are changes in social structure which are reflected in the lives of individuals as problems requiring personal adjustment. At this level such changes will continue to affect the individual scientist as he moves more and more into the role of scientific organization man.

On the societal level these changes serve to re-enforce similar changes in business, education, government, religion, and other areas of life. They are in line with increasing specialization and the organization of specialties into bureaucracies. As long as such organizations of scientists can contribute to the long-run increase in material well-being and can help to maintain American society in a world of competing and co-operating societies, present trends will probably be accelerated and the popular faith in the scientific method as the "proper" approach to solving problems will be strengthened.

The Government's Growing Recognition of Social Science

By Harry Alpert

ABSTRACT: Important new developments have strengthened the standing of the social sciences in the federal government. Historical analysis emphasizes the recency of the government's recognition of the national contributions of social science research. Significant progress has been made despite critical fluctuations. Five factors contributing to the more favored governmental position of social science research are (1) changing congressional attitudes; (2) acceptance of the social sciences at the White House level; (3) inclusion of the social sciences as part of broad definitions of scientific disciplines; (4) the general post-Sputnik interest in American education; and (5) the concern with redressing imbalances in American higher education. Research support for the social sciences is growing but a critical shortage remains in funds for fellowships and assistantships. The social sciences approach the next decade in a climate of acceptance and encouragement.

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"THEY never had it so good." This vernacular phrase may startle grammarians, but it describes accurately the present position of the social sciences with respect to support and interest by the federal government. As the result of important new developments which have served to consolidate the standing of the social sciences in the federal government, there is every likelihood that the ten years from 1950 to 1960 will be viewed as the "March" decade of the social sciences. March, according to folk weather lore, comes in like a lion and goes out like a lamb. Similarly, the nineteen-fifties may be said to have come in with a roaring antipathy to the social sciences and to be departing with attitudes of positive interest and quiet acceptance.

That it has taken so long for the federal government to develop a modus vivendi with the social sciences is quite ironical, for its involvement in social research was written into the United States Constitution. By providing for a decennial census and making this count of the population the basis for representation in Congress, our founding fathers made a social science activity the ultimate basis of political power.[1] In fact, the gathering, analysis, and dissemination of social and economic statistics has continued to be one of the three major ways in which the federal government relates itself to the social sciences. The other two are: exploitation and utilization of the findings and results of social research; and direct support of social sciences through the intramural conduct of social science research in the federal government's own research laboratories and units or through contracts and grants for extramural social science studies at colleges and universities, other nonprofit organizations, and business and commercial establishments.

PRE-WORLD WAR II STATUS

Up to World War II, the role of the federal government in the social sciences consisted largely of the first two of these functions, namely, producing mass statistical series and exploiting social science findings produced outside of the government. During the nineteenth century, the social sciences played a modest but effective role in the development of government powers and programs. Don K. Price has called attention to the contribution of economic and statistical series in the growing development of the regulation of business, as well as to the impact of John R. Commons' institutional economics on labor legislation and of Charles Francis Adams' studies on the regulation of railroads.[2]

Even as late as 1940, the government's direct activities in the social sciences were still predominantly confined to the collection and analysis of statistical information.[3] However, the roots of later developments in the government's social science programs were discernible in the 1920's. The appointment by President Hoover of a research committee on recent social trends provided significant White House endorsement of a major social science enterprise. Further impetus for governmental support of the social sciences came in the thirties from the practical programs of the New Deal. An outstanding example was the Department of Agriculture's Division of Program Surveys which assumed the leadership in introducing the sample interview survey as a basic social science tool and as an instrument of governmental policy.

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[1] See Don K Price, *Government and Science* (New York: New York University Press, 1954), p. 5.

[2] *Ibid*, pp 11–12.

[3] The several paragraphs which follow are adapted from the author's chapter on "The Growth of Social Research in the United States" in Daniel Lerner, editor, *The Human Meaning of the Social Sciences* (New York: Meridian Books, 1959), pp. 73–86.

IMPACT OF WORLD WAR II

But the defense mobilization period and World War II itself were undoubtedly the major catalytic events leading to the expansion of the federal government's programs of social science research. The events of the war on both the military and civilian fronts and the problems of postwar adjustment as they affected the nation and the individual provided the social sciences with dramatic opportunities to demonstrate their practical value and essential role in modern society. A brief review of illustrative uses of social science during World War II lists eight examples of problem areas in which important social science research accomplishments were achieved: soldier orientation and morale; analysis of command problems, particularly among Negro troops; more efficient use of psychiatry; venereal disease control; analysis of the American soldier's problems of adjustment, combat performance, and response to mass communications; evaluation of Japanese morale; estimation of war production requirements; and regulation of prices and rationing.[4] To this list may be added the media analysis activities of the Office of War Information and the Foreign Broadcast Intelligence Service; the propaganda studies of the Library of Congress, Department of Justice, and various intelligence agencies; the surveys of war bond purchases and other evaluations of the effectiveness of drives; the testing of the public comprehension of governmental information materials; and research on national character and other problems related to a better understanding of the behavioral characteristics of foreign peoples.

POSTWAR DIFFICULTIES

The immediate postwar period of demobilization witnessed the dismantling and disappearance of many of these wartime programs. Dissatisfaction with the limited accomplishments of some of these social science activities was expressed, largely as the result of the disillusionment which set in when excessive promises of achievement were unfulfilled. Social scientists became their own worst enemies by promising too much, too fast and accepting funds in excess of what could be effectively expended. Moreover, the social sciences have suffered from their minority group status among the scientific disciplines. Like minority groups on the labor market, they are subject to the rule of "last hired, first fired." Thus, many social science programs were speedily demobilized because of their relatively low priority and because of a failure to appreciate their long-range implications and future contributions.

Nevertheless, significant efforts were made to continue programs which had demonstrated their effectiveness during the war. The Office of Naval Research, created shortly after World War II, supported research on manpower problems, personnel and training, group morale, organizational structure, and related social psychological areas. The Army continued, in abbreviated form, its studies of opinions and attitudes of American soldiers. The new Department of the Air Force, proud of the accomplishments of the Aviation Psychology Program, organized units to undertake and support research in problems of selection and training, manpower, leadership, human relations and morale, and psychological warfare. When the Research and Development Board was established in the Department of Defense it included a Committee on Human Resources.

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[4] Russell Sage Foundation, *Effective Use of Social Science Research in the Federal Services* (New York· Russel Sage Foundation, 1950).

However, the skepticism and disenchantment which many of these programs engendered did not provide a favorable environment for their persistent growth and development. There set in, consequently, a period of recurring ups and downs, of "acute, and sometimes critical fluctuations," as Leonard S. Cottrell, Jr. has described it.[5] A "starts and fits" pattern became evident: an activity got started and then was curtailed or discontinued when some congressman or general threw a fit. The Division of Research of the Housing and Home Finance Agency, the excellent survey research unit of the Veterans Administration, the Air Force's Human Resources Research Institute at Maxwell Field and its Personnel and Training Center at Lackland Air Force Base were but a few of the research units which experienced difficulty.

Despite the "on again, off again" character of some of these programs, the long term trend was toward increasing appreciation of the social sciences as valuable national assets. As the postwar pattern of extramural support developed, the social sciences, too, received encouragement, although not at the same rate and magnitude as the physical and life sciences.

THE "MARCH" DECADE

The "March" decade, 1950–60, will perhaps be viewed historically as the turning point in federal government recognition of the social sciences. The full measure of the change from the "lion" to the "lamb" phase of this decade may be observed in comparing the National Science Foundation Act of 1950 with the National Defense Education Act of 1958. In the former legislation, the social sciences are included only on a permissive basis and are referred to only as "other sciences." In the 1958 act, the section dealing with graduate fellowships mentions no limitations whatsoever with respect to disciplines. Moreover, a separate title provides for research and experimentation in more effective utilization of television, radio, motion pictures, and related media for educational purposes. This act also recognizes the importance of improving statistical series in the field of education.

Note must be taken, also, of other evidences of changing attitudes toward the social sciences, such as the establishment, in December 1958, of an Office of Social Sciences within the National Science Foundation; the appointment, in the Spring of 1959, of a sociologist, President Logan Wilson of the University of Texas,[6] as a member of the National Science Board; and the expansion of the social science research activities of the Department of Health, Education, and Welfare.

In the vernacular of the boxing ring, it may be said that the social sciences were, for several years, definitely "rocky and punch drunk," but were still on their feet when the fight was over. They have survived Cox Committee and Reese Committee investigations. They have endured pariah status and innumerable reorganizations. They have weathered appropriation storms which threatened to cut off funds for studies of child-rearing practices, mother-love among lambs, population dynamics, message diffusion, and other projects which became the pet peeves of individual legislators.

MAJOR DYNAMIC FACTORS

In attempting to assess the major factors that account for the more favorable position in which the social sciences find themselves at the end of this decade, I am able to identify five important considerations: (1) changing Congressional attitudes; (2) acceptance of the social sciences at the White House level; (3) inclusion of the social sciences as part of broad umbrella definitions of scientific disciplines; (4) the general post-Sputnik interest in American education; and (5) the concern with redressing the imbalances in education which stemmed from the earlier almost exclusive emphasis on natural science and mathematics. Brief comments on each of these five factors follow.

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[5] Leonard S Cottrell, Jr, in Foreword to Morris Janowitz, *Sociology and the Military Establishment* (New York· Russell Sage Foundation, 1959), p. 5.

[6] Dr. Wilson was subsequently required by Texas law to give up his membership on the National Science Board.

CHANGING CONGRESSIONAL ATTITUDES

In his report on the crucial Senate debate in 1946 which preceded the vote to exclude from the then pending bill to establish a National Science Foundation the specific provision which created a Division of Social Sciences, George A. Lundberg concluded that the Senate thought of the social sciences as at best "a propagandist, reformist, evangelical sort of cult."[7] The unfortunate phonetic confusion of social science with socialism reinforced such viewpoints. Just a few years later, however, more positive attitudes were being expressed. In 1953, the Cox Committee, in its "Final Report," noted the special importance of the social sciences in the contemporary world. It stated:

> It is entirely possible that in a time when man's mastery over the physical sciences threatens him with possible extermination the eventual reward from the pursuit of the social sciences may prove even more important than the accomplishments in the physical sciences.[8]

Other important turning points in Congressional expressions toward the social sciences were the vigorous statements by Senator Estes Kefauver's Subcommittee on Juvenile Delinquency in 1955, 1956, and 1957; the 1955 recommendations of Representative Richard Bolling's Subcommittee on Economic Statistics of the Joint Committee on the Economic Report; Senator Hubert Humphrey's report to the Senate in 1957 of his experiences in the Middle East; and speeches by Senator Wayne Morse, Representative Charles O. Porter and others.[9] This year neither house of Congress raised any objections to the National Science Foundation's request for $2,000,000 for support of basic research in the social sciences in fiscal year 1960, even though this represented a considerable increase over the $850,000 appropriated for this purpose for fiscal year 1959. (The actual budgetary allowance for social science research in the National Science Foundation for fiscal year 1960 is $1,600,000.)

This is an encouraging picture, indeed. But Congressional confusion regarding social science has by no means been completely eliminated. Negative attitudes still persist and need to be reckoned with.[10]

WHITE HOUSE INTEREST

The White House, too, has shown increasing interest in the support of the social sciences. In his State of the

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[7] "The Senate Ponders Social Science," *The Scientific Monthly*, Vol. 64, No. 5 (May, 1947), p. 399.

[8] "Final Report" of the Select Committee to Investigate Foundations and Other Organizations, 82nd Congress, 2nd Session, House Report No. 2514, Union Calendar No. 801 (Washington: Government Printing Office, January 1, 1953), pp. 9–10.

[9] For details and references, see Harry Alpert, "Congressmen, Social Scientists, and Attitudes Toward Federal Support of Social Science Research," *American Sociological Review*, Vol. 23, No 6 (December, 1958), pp. 682–686.

[10] See, for example, *Independent Offices Appropriations For 1960*. Hearings Before the Subcommittee of the Committee on Appropriations, House of Representatives, 86th Congress, 1st Session (Washington: Government Printing Office, 1959), p. 527. For a discussion of persisting negative attitudes, see Harry Alpert, *op. cit.*

Union message delivered on January 9, 1959, President Eisenhower expressed his desire to undertake a systematic study of American values, goals, and social trends, comparable to the earlier Hoover Committee study.

The objective, President Eisenhower said, would be "the establishment of national goals that would not only spur us on to our finest efforts but would meet the stern test of practicality." He hoped that this new study would be concerned, among other things, "with the acceleration of our economy's growth and the living standards of our people, their health and education, their better assurance of life and liberty and their greater opportunities." He noted that the report of Hoover's Recent Social Trends Committee "has stood the test of time and has had a beneficial influence on national development." Here, indeed, is a significant compliment to social science.

And in its report on "Strengthening American Science," issued December 27, 1958, the President's Science Advisory Committee included social psychology among the scientific disciplines for which a strong case could be made for intensifying the nation's scientific effort. The Committee stated, "And advances in social psychology might help to reduce tension and conflict at every level of human intercourse—in our communities, in business and industry, in Government, and even among nations."[11] Furthermore, as previously noted, President Eisenhower has appointed a social scientist to the National Science Board. This policy-determining body for government science on January 23, 1959 adopted the following statement:

> The National Science Board recognized the importance, as well as the complexity and difficulty, of research in the social sciences It is clear that the intellectual, economic, and social strength of our Nation requires a vigorous approach to social problems, with scientific techniques of study making their maximum contribution.[12]

PROTECTIVE UMBRELLAS

The social sciences have prospered best in the federal government where they have been included under broad umbrella classifications of the scientific disciplines such as agricultural sciences, military sciences, medical sciences, and health sciences. Under such umbrellas and in close company with scientific areas which enjoy the prestige and status of biological or physical sciences, the social sciences have enjoyed a protection and nourishment which they normally do not have when they are identified as such and stand exposed, "naked and alone."

Agricultural research has been heavily supported by the federal government from its very inception. Quite early the concept of agricultural sciences was broadened to include not only biological research but agricultural economics and rural sociology as well. In fact, for many years the Department of Agriculture's Bureau of Agricultural Economics was internationally famous for its leadership in significant areas of social and economic research. Although from time to time specific social science projects of the Department of Agriculture have suffered Congressional attack, there has been little question of the legitimacy of the inclusion of social research in the scientific program of the Department. In fact, one appropriation committee, with remarkable indifference to the distinction between biological and social science research, once included, in a list of fields for which research funds were not to be expended, the orchids of Guatemala, the flora of Dominica, child-rearing practices, research methodology, and population dynamics.

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[11] *Strengthening American Science:* A Report of the President's Science Advisory Committee (Washington· Government Printing Office, 1958), p. 4.

[12] Reproduced in *Congressional Record* by Representative Charles O Porter, March 10, 1959, pp. A 1969–70.

The Medical Sciences and Health Sciences rubrics have also provided generous hospitality to the social sciences. Social science research projects are given careful and sympathetic consideration by at least five study sections of the National Institutes of Health: Behavioral Sciences, Hospital Facilities Research, Mental Health, Nursing Research, and Public Health Research. Social scientists serve as members of these study sections as well as on several other committees of the National Institutes of Health. The National Institute of Mental Health's Laboratory of Socio-Environmental Studies is outstanding in the quality of its research program.

Research undertaken by the military establishment in relation to the defense needs of the nation develops strong immunities to Congressional or other attacks if military authorities certify its importance to the mission of the Department of Defense. Despite the ups and downs previously referred to, the Army, Navy, and Air Force have arrived at a realization of the importance of basic research in the social sciences. The Office of Naval Research includes a Psychological Sciences Division. The Air Force has established a Behavioral Sciences Program in its Office of Scientific Research. And here is the testimony of an Army General presented recently before an appropriations committee:

> We can never afford to neglect basic research and the Army wants to do more of it whenever we find applicable projects to further this increase of scientific knowledge. Such research is not confined to the physical sciences. Investigation of the social sciences to help us to utilize more effectively our manpower and insure man-machine compatibility with complex engines of war being developed is vital. Should we neglect these important considerations we only aggravate the trend in which the physical sciences are outstripping the social sciences and may, in time, reach a point where the machine may destroy its maker.[13]

These are the words of Lieutenant-General Arthur G. Trudeau, Chief of Research and Development, Department of the Army.

Another important umbrella for the social sciences is Operations Research. The various operations research units supported by the federal government have invariably included a social science component.

IMPACT OF SPUTNIKS

The social sciences have not been indifferent to the whir of the Russian Sputniks and have directly felt the impact of these successes in space technology. It was recognized that Soviet Russia's accomplishment was not only the result of advances in science and engineering but also the consequence of a social system that was capable of making and carrying out significant decisions. Interest developed in studies of the social, economic, and political implications of the space age. It became imperative that we keep ahead of the Russians in the social science fields. For this reason, Vice-President Richard M. Nixon encouraged the formation of a committee on National Support for Behavioral Science which reported on social science needs to the President's Scientific Advisory Committee. Substantially increased appropriations were made available to the National Science Foundation, and in the National Defense Education Act of 1958, Congress officially declared as national policy the doctrine that the defense of this nation depends upon the mastery of modern techniques developed from complex scientific principles, and, as well, upon "the discovery and development of new principles, new techniques, and new knowledge."[14]

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[13] *Department of Defense Appropriations For 1960.* Hearings Before The Subcommittee on Appropriations, House of Representatives, 86th Congress, 1st Session (Washington: Government Printing Office, 1959), p. 339.

REDRESSING IMBALANCES

For a time, it looked as if only the natural sciences and mathematics would be the beneficiaries of the increased responsibilities of the federal government toward research and education. Programs were quickly organized to improve the quality of science teaching, to train more scientists and engineers, and to intensify the pace of research in the physical, mathematical, and biological sciences. It became evident, however, that the neglect of other areas of scholarship and learning would spell national disaster. The government's difficulties in international relations led to intensified interest in language study. Soon voices were heard calling attention to the need to redress the imbalances in American education which a predominant concern with the natural sciences and engineering was creating.[15] Cognizance of this requirement is found in the newly released report of the President's Science Advisory Committee on "Education for the Age of Science." This report stresses the fact that, "Today in America we need a very wide variety of human talents."[16] It goes on to urge that "a proper balance be maintained in our educational offerings."[17] To achieve such a balance we must encourage intellectual leadership in the humanities and social sciences as well as in the natural sciences and mathematics.

HEALTHY PROGNOSIS

The social sciences thus face the nineteen-sixties in an atmosphere of encouragement and with the active support of influential well-wishers. Research funds are becoming more plentiful. The federal government alone will soon be spending in the neighborhood of $60,000,000 a year in support of the social sciences. This estimate does not include the $100,000,000 or so that the Decennial Census of 1960 will cost.

MORE FELLOWSHIPS NEEDED

A major problem, however, remains. The most urgent need of the social sciences is expansion of the pool of available trained, specialized manpower. Recent studies have indicated that the length of time required to obtain the Ph.D. degree is strongly influenced by the availability of financial support to graduate students in the form of assistantships and fellowships. It is here that the social sciences, and humanities, too, are most seriously disadvantaged vis-à-vis the natural sciences. The major bottleneck in the advancement of the social sciences is not research funds, but fellowship and scholarship opportunities for basic and advanced training. If the social sciences are to fulfill the general public's expectations of them, they must double, at least, the number of trained practitioners. To make the training process more productive and more effective, however, additional fellowships and other types of financial support for training are an imperious and critical necessity. Title IV of the National Defense Education Act has been extremely helpful in this regard. Almost a fourth (23 per cent) of the first 1,000 graduate fellowships were awarded in the social sciences. The various training programs of the National Institutes of Health also provide valuable opportunities for social science education. But more needs to be done. The National Science Foundation, for example, has the basic legislation to include the social sciences within its "Education in the Sciences" program. It also has reasonably adequate funds for training and education. It has broadened its conception of the social sciences in its research support program. Only administrative nearsightedness prevents it from giving the social sciences, broadly conceived, their deserved place within the various program activities of its Division of Scientific Personnel and Education.

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[14] Public Law 85-864, Section 101.

[15] See, for example, statements by Pendleton Herring and Harry Alpert in the February 1, 1958 issue of *The Saturday Review* (Vol. 41, No. 5).

[16] "Education For The Age Of Science." *President's Science Advisory Committee* (May 24, 1959), p. 3.

[17] *Ibid.*, p. 6.

COMPLACENCY TO BE AVOIDED

We can be proud of the achievements of the social sciences in government, but we cannot afford to be complacent. Certain past mistakes must be avoided: premature promises, excessive expectations, hasty growth, disastrous indifference to the political process, unwarranted impatience with the administrative processes of justification and review, and lack of concern with the public image of the social sciences. By careful planning and effective operations a solid basis can be established for future growth.

Advance in the social sciences will depend most immediately on what in fact social scientists do: how well they teach at the undergraduate level, how well they communicate with the general public, how effectively they respond to calls from industry and government for help in resolving practical problems, and how much they devote to fundamental research. It depends also on their willingness to cultivate patience and humility.[18] Charles Dollard has well defined the problem: "The long-term contract of the social scientist with society is not to perform miracles but to bring to the study of man and his problems the same objectivity and the same passion for truth which have in the past given us some understanding and control of the physical world."[19]

[18] See *The Saturday Review,* Vol. 41, No. 5 (February 1, 1958), p. 38 and *The Saturday Review,* Vol. 42, No. 14 (April 4, 1959), p. 64.

[19] "Strategy For Advancing The Social Sciences," in Social Science Research Center of the Graduate School, University of Minnesota, *The Social Sciences At Mid-Century* (Minneapolis: University of Minnesota Press, 1952), pp. 19–20.

Special Problems of Military Research and Development

By Willis H. Shapley

ABSTRACT: Many of the problems of military research and development are not special, either because they are common to other comparable research and development programs or because they are common to other parts of the military programs. The military programs are concerned with research, but the main emphasis is on development and on the entire chain of activities necessary to improve the capabilities of the operational forces. The magnitude and complexity of the programs are tremendous; these factors, particularly the complex nature and interrelations of their objectives, are at the root of most of the problems of military research and development. The basic organizational problem is to find a way to centralize responsibility for the crucial decisions involving selections among alternative military capabilities and technical approaches, while at the same time decentralizing detailed management to allow a maximum of freedom and flexibility in the actual conduct of research and development work. The problems of military research and development culminate in the budget process. Making the proper allocation to military research and development within the over-all limitations and distributing this amount to individual research and development projects are the most difficult problems of our time.

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PARADOXICALLY, the most useful way to open a discussion of the special problems of military research and development seems to be to consider the contrary proposition that there are no special problems of military research and development. Two principal lines of argument can be advanced to support such a proposition, and while they may not in the end be fully convincing, they carry a large measure of truth and will serve to put into perspective the problems of military research and development, be they special or otherwise.

One can argue with some cogency that what appear to be special problems of military research and development are, for the most part, problems in nonmilitary research and development as well, and that they become identified with military research and development because their counterparts in nonmilitary research and development are not so conspicuous or so well publicized. The many problems resulting directly or indirectly from the sheer size and complexity of the military research and development programs cannot be fairly charged either to the military content or to the military environment of the programs. Corresponding problems of large-scale administration exist in varying degrees in the larger nonmilitary programs, and the same bureaucratic frustrations for which the military agencies are traditionally blamed are all too often encountered in civilian government agencies, in industry, and even in universities. Shortages of first-rate scientific and technical personnel are a continuing problem for nonmilitary and military programs alike. Even some of the problems scientists have with military secrecy and security have counterparts in proprietary research in industry. The counterpart of wasteful interservice rivalry is usually called healthy competition.

Taking a different approach, one can make a strong case that many of the problems of military research and development are not peculiar to research and development, but are manifestations of broader problems of military policy or management. The difficult problems that arise from the interplay between technical possibilities and military requirements are fundamental problems of military planning, and involve strategy, tactics, and logistics as well as research and development. The budgetary problems of military research and development are a part of the broader problems involved in the total military budget, both in the determinations of the total amounts and in the decisions on complicated and interrelated details. Instances of duplication and confused or overlapping jurisdiction in military research and development are for the most part the result of deeper problems of roles and missions and arise from inherent complexities in the variety and interdependency of military requirements. Problems of administrative competency and technical qualifications and training of military personnel are not limited to the research and development area. The reluctance to discontinue programs which have passed their period of usefulness, one of the principal present problems in military research and development, was also evident in the persistence of the cavalry and the battleship.

Considerations such as these do not prove that there are no special problems of military research and development, but do suggest what is perhaps the main conclusion of this article, that the principal problems of military research and development in the United States are either inherent in the conduct of a large-scale organized research and development effort or, more importantly, the consequence of the dynamics and dilemmas of the broader problems of national defense we face today and in the years ahead.

RESEARCH AND DEVELOPMENT

It is advisedly that we are speaking of research and development, not just research. The spectacular and well-publicized successes of scientists in World War II and the importance which has rightly been attached to establishing broad and continuing support for basic research have occasionally had the effect of distracting attention from the importance of development, and have created somewhat the impression that scientific research by itself produces national or military strength. It has long been customary to justify additional expenditures for basic research on the grounds of its contributions to national strength, and more particularly, except perhaps in the medical field, to national defense.

At the same time it is fashionable in scientific circles to deplore the large sums going into development, especially for military weapons and equipment, and to urge a redistribution of budgetary wealth in favor of more research. This suggestion may have merit in particular cases, but the notion that there should be more emphasis on research and less on development as a matter of principle is clearly fallacious. To the extent that research is justified by possible practical applications, military or other, the role of development must be recognized, not as a menial adjunct to the higher efforts of pure science, but as an equally essential link in the chain of achievement.

In military research and development, the primary concern is with development, but the chain does not stop there. Newly developed weapons and equipment must be tested and evaluated, both for technical performance and for suitability for use by military units. Appropriate tactics and employment concepts must be evolved; combat troops and maintenance personnel must be trained in the intricacies of the new equipment; and sufficient quantities must be produced for deployment or stock-piling. And there are other vital links at the other end of the chain. Operational military requirements must be carefully analyzed in the light of technical possibilities and possible countermeasures within an over-all strategic framework. Before specific development can begin there must be design and feasibility studies.

In all this effort, scientific research plays an important and essential role in providing new knowledge, ideas, and techniques. It is a necessary part of the process, but not sufficient to achieve the goals. In an important sense, the prime problem of military research and development is to make the entire developmental process work from pure research and analysis to practical operational equipment in the hands of trained troops.

MAGNITUDE AND COMPLEXITY

The magnitude and complexity of the present military research and development programs are difficult to comprehend. Using dollars as the most convenient yardstick, one can identify a total of almost $6 billion of the Department of Defense fiscal year 1960 budget as going for research and development expenses of one sort or another, including expenses of the directly related activities of test and evaluation, but excluding production of new items for equipping and training operational forces or for stock-piling. This total—more than one-seventh of the total military budget—can be regarded, in round numbers, as being made up of about $700 million for research, about $4,800 million for development, including test and evaluation, about $200 million for pay and allowances of military personnel, about $150 million for construction of facilities, and another $150 million set aside as an emergency fund for unforeseen requirements in the research and development field.

In addition to all this, substantial but hard-to-define portions of the research and development activities of the Atomic Energy Commission and the National Aeronautics and Space Administration are essential parts of or directly support the military research and development programs. In total, military research and development currently accounts for approximately 80 per cent of the government's total annual outlay for research and development, and provides the financial support for a little over one-half of all the research and development in the United States.

Some aspects of the complexity of this vast effort on military research and development can be seen in the scope and variety of the activities it encompasses. There is basic research in almost all branches of the physical, engineering, life, and behavioral sciences, including theoretical work, laboratory research, and large-scale experiments, such as those involving high altitude balloons, satellites, or polar expeditions. There is applied research and technical development in almost every field of technology—metallurgy, electronics, propulsion, and so on—so various are the requirements and problems of the military development programs. The development programs themselves cover thousands of different kinds of items, ranging from prosaic but vital improvements to paints and preservatives to fantastically complex aircraft, missile, and electronics systems. Test and evaluation programs may, at one extreme, involve only simple, small-scale tests on a shake-table in a laboratory; at the other extreme, the test and evaluation of each new type of aircraft and missile requires a major manufacturing effort to produce dozens of costly test articles and the use of elaborate test facilities and ranges stretching as much as a quarter of the way around the world.

Then there is the great variety in the methods by which military research and development activities are carried on. There are many different types of government laboratories, development centers, and test installations, some operated directly by the government with civil service and military personnel, others by operating contractors, and a few by a mixture of both. All sorts of industrial concerns, universities, and other private institutions perform military research and development by contract. There are many different kinds of contractual arrangements, and the contracts themselves range all the way from a few thousand dollars for several months of theoretical research by one or two university scientists, to hundreds of millions of dollars a year for the development of a major weapons system by a prime industrial contractor with a whole host of subcontractors and associates.

OBJECTIVES

However, the most complex aspect of the military research and development programs lies in the relationships of these programs to their objectives. This type of complexity is at the root of most of the problems of military research and development.

In general, the ultimate objective of the military research and development programs is to provide or enhance military capabilities in support of national military policy. This is easily said; the difficulties arise in translating such a general statement into specific objectives for individual research and development programs. The ends of national military policy are not always clear, change from time to time, and are often controversial.

Even if one assumes general agreement on certain basic objectives—such as (a) maintaining an offensive and defensive military posture that will deter a Russian attack on the United States or its allies and (b) maintaining an adequate capability for dealing with limited war situations that are likely to arise—the range of possible objectives of military research and development is not appreciably narrowed. Many different types of military capabilities can contribute to each of these basic objectives; many different kinds of weapons systems can be conceived to provide each type of military capability; and many different technical approaches can be followed in developing each kind of weapons system. Each weapons system development presupposes or requires the development of components; the development of components depends on general technical development; and technical development in turn is based on principles and possibilities that have emerged from applied and basic research.

For these reasons, the objectives of the thousands of separate projects in the military research and development programs cannot be simply and directly related to national policy objectives. In the case of the development programs, the projects fall into groups comprising alternative technical and military approaches for the same or related objectives. At the research end of the spectrum, it is more a matter of attempting to anticipate the nature of future developmental requirements and choosing the lines of basic and applied research which appear most likely to uncover technical possibilities of future military significance.

Thus the establishment of objectives for both research and development involves difficult problems of selection among alternatives which themselves are seldom clear-cut. The fact that the entire complex of interrelationships is continually changing as time goes by—with changes in the international situation, intelligence estimates, military requirements, technical evaluations, scientific knowledge, and so on—further complicates the problems of selection and creates the need for continuous reappraisal of the relative contributions the various projects can be expected to make to the ultimate objectives. To working scientists and to enthusiasts for particular programs, the repeated reappraisals and the resulting frequent changes in programs and objectives often seem to be an unnecessary bureaucratic harassment, but from the standpoint of the management of the programs as a whole, they are a painful necessity.

ORGANIZATION

One of the most serious problems raised by the size and complexity of the military research and development programs is how the effort should be organized. It is obvious that so large an enterprise cannot, as a practical matter, be planned and administered in detail from one central point, such as the Office of the Secretary of Defense. It is also clear that to promote and secure the advantages of initiative and imagination on the part of creative scientists and engineers, a high degree of freedom and flexibility should be allowed to laboratories and other organizational levels directly concerned with actual scientific research and specific development projects.

These considerations point in the direction of a highly decentralized organization. The answer is not so easy, however, because, as we have just seen, military research and development is so much a matter of selecting among alternative approaches. The judgments involved in making these choices, and the appraisals of the relative importance of the military requirments on which they are based, cannot be made unilaterally by people or organizations that are directly concerned with only a part of the problem and are not in a position to appraise all of the alternatives in relation to the pertinent over-all policy considerations.

The complexity of the program, sketched above, is such that to be made properly many of these decisions have to be made, or at least carefully guided and reviewed, at very high levels in the hierarchy—at the headquarters levels of the military departments, for example. With the increasing variety of alternative technical possibilities cutting across the historical organizations and missions of the Army, Navy, and Air Force, the situation is developing more and more to the point where almost every significant technical program choice or decision has ramifications which transcend any one military service, and thus requires appraisal, decision, and perhaps active direction or co-ordination from the broad central point of view of the Office of the Secretary of Defense.

This is the organizational dilemma. Too much central direction will lead to an impossible bureaucracy that would stifle progress. Too little central direction will permit competing alternative programs to run wild on a unilateral basis. Scientific discoveries and technological break-throughs cannot be decreed by the Pentagon, but, on the other hand, crucial national decisions on choices among weapons systems should not be left by design or default to those down the line who have less than the full scope of responsibility and competence for making them.

In theory, the answer is obviously to centralize only the decisions that have to be made at the top and decentralize everything else. The practical difficulty is that merely to identify on a current basis the problems that need to be resolved at the top requires a complete and continuing review of the entire program on a fairly detailed basis. The central staff organization required for these purposes is necessarily large, and inevitably gives rise to knotty problems of internal organization and co-ordination of the central staff itself. These problems are formidable and discouraging, but should not be forever insurmountable. It is to be hoped that the evolution of the office of the Director of Defense Research and Engineering, established by the Department of Defense reorganization act of 1958 as the latest attempt to improve central leadership and control of military research and development in the Department, will demonstrate that a balance can indeed be achieved which will give at the same time both more effective central control of major decisions and more decentralized responsibility for detailed management and actual conduct of research and development work.

Role of civilian agencies

The question is sometimes raised whether it would not be better to have military research and development performed by a civilian agency outside the Department of Defense. The success of the Office of Scientific Research and Development (OSRD) during the Second World War, the contributions made over so many years by the National Advisory Committee for Aeronautics, and the present important role of the Atomic Energy Commission can each be cited as different ways in which significant aspects of the military research and development program have been carried on by civilian agencies.

It will never be possible to re-create the environment in which the Office of Scientific Research and Development made its contributions. It is no longer a question of mobilizing science for defense purposes—the present annual peacetime level of effort is already much greater than the total of all OSRD expenditures during World War II—and it is doubtful whether in any future major war we will have the time and safety to do research and development on weapons and equipment that will influence the outcome.

The success of both the Atomic Energy Commission and the National Advisory Committee for Aeronautics in operating large government-controlled laboratories is at least partly attributable to their civilian setting. At the same time, the fact that each of them has operated in areas of specialization which, while highly important, are relatively narrow compared to the total range of military research and development activities, has kept the task of co-ordination with the purely military programs within manageable limits. By contrast, the National Aeronautics and Space Act of 1958 did not give the new National Aeronautics and Space Administration a clearly defined role in relation to the "space" activities of the Department of Defense, and the initial experience has been that the problems of co-ordination have tended to offset the hoped-for advantages of a separate civilian agency.

The establishment early in 1958 of the Advanced Research Projects Agency as a civilian organization within the Office of the Secretary of Defense represents a new approach which provides a mechanism whereby military research can be done under full control of the Department of Defense but without assigning it to any one of the three military departments. It is too early to say whether a new pattern is being established or to appraise its merits, but it seems clear that the future evolution will be in the direction of centralizing responsibility for military research and development in the Secretary of Defense and his Director of Defense Research and Engineering.

For the foreseeable future the importance of close integration of military research and development with other aspects of the military programs and the need for authoritative central determination of military research and development objectives will undoubtedly outweigh the advantages that might be gained through the establishment of a separate civilian agency for military research and development.

THE BUDGETARY PROBLEM

Of all the problems of military research and development, the ones that generate perhaps the most controversy and confusion are those that involve, in one way or another, the budget. To some, these all seem to boil down to a very simple problem with a very simple answer: not enough money is the problem, and more money, the answer. Of course, one of the basic budgetary problems is always the question of how much money is enough, and the answer always at some point comes down to a matter of judgment. But the question seldom if ever arises in such simple terms, and the most difficult problem of all is to identify the areas where such judgments have to be made and to define the problems in such a way that the relative merits of each claim for money can be evaluated. This, of course, brings us back to the problems of reappraisal and selection discussed above.

An important point, which is often lost sight of and misunderstood, is that the apparent preoccupation with budgetary problems at all levels of the military research and development program does not stem from an exaggerated concern for money, economy, or bookkeeping as such, but from the fact that virtually all of the substantive and policy problems of the programs necessarily have significant budgetary implications. Budgetary decisions are one of the principal regular administrative mechanisms for making substantive program decisions.

This is not to say that financial limitations do not play an important part in the budgetary problems of the military research and development programs. Over-all limitations on the total budget do in fact exist at any given time, and even though they are not necessarily expressed as rigid ceilings in specific dollar amounts, they have to be taken as the framework within which the forward planning of all federal programs, civil as well as military, proceeds.

As a practical matter in the development of the federal budget, once the general magnitude of the over-all totals has been tentatively determined—a major decision of national policy based on consideration of the urgency of the demands on the budget and the economic and political implications of prospective alternative budget totals—it is necessary to establish tentative planning figures for the principal agencies, including the Department of Defense. These planning figures guide the preparation of detailed budget proposals which show the impact on the programs of the assumed planning figures and permit the consideration of alternatives, including, if necessary, reconsideration of the initial planning figures themselves.

Impact of limitations

In this situation, it quite naturally seems inconceivable to the managers or proponents of any single military research and development program that it is really their project that has to suffer in the budget decisions. It is hard for them to see why their sometimes quite modest requirements cannot be met by a slight adjustment in other programs without changing the planning totals, or, failing that, why the economy of the country would be ruined by such a relatively small increase in the over-all totals.

It is true that the immediate impact of any particular budgetary restraint can be alleviated either by "robbing Peter to pay Paul" or by a decision to increase the total by the amount required. The first course merely results in a different set of decisions in the selection process. The second represents a chipping away at the economic and political judgments which led to the planning figures in the first place, and the question of how rigidly they are to be adhered to is again a matter of economic and political judgment.

But neither course will satisfy all claimants, especially in the military research and development programs. The almost unlimited proliferation of military requirements and technical approaches, together with the inexorable increases in cost as projects move from research into development, will conspire to generate pressures for additional funds within any foreseeable plausible total budget. The distribution of the total budget between military and nonmilitary, the division of the military budget as between research and development and the other essential costs of national defense, and finally the selective allocation of research and development funds to the myriads of individual projects, taken together, constitute one of the most formidable and awful problems of our time, and one which should command the best efforts of the officials and scientists who have to deal with any of its many ramifications and the sympathetic understanding of the public.

Budgeting for Science: Presidental Responsibility

By William D. Carey

Abstract: "Science," as it is categorized without verbal exactitude, is a new and troublesome concern of the Presidency. Executive oversight of science as it impinges on public policy is in part exercised, and can be appraised, in the context of budgetary decision-making. Political scientists will find it rewarding to ponder the adequacy of the Presidential machinery for evolving science policy and balancing priorities within the discipline of the federal budget. Can the scientific community reasonably look to the federal government for the degree of sufficiency, stability, and venturesomeness of support which it seeks, given the competition of other urgent public needs for budgetary sustenance? Upon reflection, the institution of the Presidency for choosing priorities and tackling vexing technological questions is found to contain reassuring strengths. At the same time, preoccupation with the snarled traffic of relentless decision-making leaves unresolved a number of basic, long-term problems in the design of the relationship of science to government.

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LAST May, before a blue-ribbon audience of scientists and business and philanthropic leaders gathered at the Waldorf-Astoria, the President prefaced a major address on science with the greeting: "Fellow Innocents." Though said in jest, the words were indicative of the problems of the Presidency in establishing the rightful place of science as a new and at times bewildering partner of the national government.

Few aspects of public policy afford such tantalizing opportunities for exploration and discovery as the interaction of government with science and technology. We are still on the fringes of understanding the significance and future implications of this trend. We are, indeed, fellow innocents.

Popular attitudes cloud rather than illuminate the government's search for a comprehensive policy toward its role in science. When science is viewed as an all-absorbing investigation of the basic nature of man's environment, it plays to nearly empty seats. But if science is a subpolar pageant, or a multimillion-dollar effort to hurl a mouse into outer space, it acquires a curiously high value. The distinctions between science and technology, between research and spectacular demonstration, are still unclear, and so they are all vaguely fused into "science." The beginning of wisdom would necessitate defining some terms.

FEDERAL SUPPORT OF SCIENCE

Budgeting for science (and technology), like all budgeting, is an art of making choices, hopefully if not precisely conforming to the scale of contemporary social values. When the desirable equilibrium between resources and requirements is lacking, or, more baldly, a shortage of funds exists to satisfy all petitioners, this process of choosing becomes a very considerable undertaking. In the national government, the institution of the Presidency assumes much of the responsibility for the fiscal decisions which determine the direction and distance of public policies. The Presidency has, accordingly, been a target for criticism on the part of those who have felt, at one time or another, that the budget did not provide enough support or assurance of stability for science.

How much support is "enough"? That is a question by no means the monopoly of the advocates of science. It has been raised in every area of public policy, and will always be raised because the nature of budgeting produces what might be called a fiscal law that dissatisfaction shall be distributed uniformly. As for "stability," no one can be against it as an objective to be pursued, but the fact must be faced that when science becomes an instrument of public policy, it serves an essentially unstable master. To seek optimum stability through so dynamic and unpredictable a device as the budget, is to expect more than it can deliver In such a setting, science runs some calculated risks which are basic to the political process. The same public opinion which urges greater scientific and technological achievement is capable of simultaneously demanding economy in government, along with tax relief.

The inescapable fact is that the policy-makers in the national government, including those in the executive branch, have no special intuition to guide them toward absolute values where science and technology are concerned. Science, Dr. Vannevar Bush has said, is "the endless frontier," and it follows that any level of budgetary support is at best only relative justice. Like everything else associated with the equities of budgeting in the national government, the claims and aspirations of science must run the gauntlet of relative priorities in a setting of acute competition for funds, and under the handicap of a "language barrier." At the same time, we cannot view the role of science and technology in our affairs by the comparatively simple standards of an earlier and less complex era.

All things considered, science and technology have not fared badly in the budgetary sweepstakes. Only two decades ago, federal support for all science was at the level of 100 million dollars annually. In the budget for fiscal year 1960, a special analysis reveals that expenditures under the head "research and development" will reach 5.5 billion dollars. Of course, these are not wholly comparable figures, but they do have comparative meaning. The clue to all this prosperity is, of course, suggested by the fact that over four-fifths of the 1960 amounts are directed primarily to the needs of national security, and the totals are heavily distorted by the fantastic costs of hardware development.

Even so, the blush is not entirely off the rose. The same budget analysis brings out the additional point that at present the federal government supports about half the research and development of the nation. "Science" is doing very well at the hands of government, if the aggregates can be taken at their face value.

But can they be? Do the sheer weight and built-in fertility of research and development effort on this scale really control the final totals which appear in the budget, or is there an apparatus of decision-making which lends a reassuring tone of deliberate choice and validity to the astronomic results?

"Reasonable austerity"

Even the scientific community itself is not wholly without apprehension in this regard. While its general tendency is to rejoice in the latter-day miracle of the loaves and fishes, an occasional dissenter rises up with a minority report. Such was the case at a recent symposium on basic science, when Dr. Merle Tuve of the Carnegie Institution of Washington spoke of the dedication to pure research which characterized those private research institutions established many years ago "when there was less financial and technical exuberance, with regard to the support and administration of research effort, than we see all about us today." And he observed that "suitable provision for the recruitment (of the lively individual investigator) and his welfare and increase in his number is unquestionably of much greater importance than the provision of spectacular facilities carefully calculated in tons and dollars by the business men and politicians of science surely to outdo the Russians in the same field."[1]

Dr. Tuve then proposed the guideline of "reasonable austerity" to protect the researcher from becoming a manager or "big operator." As Merle Tuve explained:

> Austerity as an essential requirement for creative research may sound harsh and out of date to most of the big operators and many of the young technical men in research today. For our discussions, however, I wish you would just recall to yourselves some of the examples of finished basic research which you most honestly respect and then answer the austerity question silently for yourself. . . . The undeniable fact is that creative work is nearly always done with limited resources and under difficult conditions. I suggest that this austerity is to a considerable extent a necessary discipline for the critical faculty and a basis for the Spartan choices which must be made by the investigator as to emphasis and deletion in the course of any basic research. . . .

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[1] "Basic Research in Private Institutes," Carnegie Institution of Washington, preliminary draft of paper read to the Symposium on Basic Research, New York City, May 15, 1959.

This is quoted at some length, though only in capsule, not as an illustration of the majority view but, to use Dr. Tuve's own words, as "a little mustard." It is the sweet relish, however, that is more popular. Spartan austerity is possibly an acceptable abstraction to which many would subscribe but few would be prepared to embrace, in or out of the scientific fraternity.

DECISION-MAKING PROCESS

It is inevitable that as the costs of research and development assume large and increasing proportions, while federal fiscal policy attaches a central importance to balanced budgets, the criterion of budgetary priority looms larger in the decision-making process. This is true not only in the vast and troublesome area of weaponmaking, but even in the "austerity" zone of basic research. For example, some of the most tempting adventures in basic science are to be found in the field of high energy physics, but it is a costly path to follow when the instrumentation turns out to be a two-mile linear accelerator the construction price of which is more than 100 million dollars, to say nothing of annual operating expenses.

This illustration affords us a useful peep-hole into the anguish of decision-making. Here is only one facet of basic science, albeit a strategic one. The entire scale of government-supported basic research is in the vicinity of one-half billion dollars, if the statistics can be trusted to yield a credible measurement. Across the wide spectrum of the subject matter of basic research, the plea is made for greater support. Possibly, the fertilization of the entire area through spreading another 100 million dollars would provide a massive tonic to American science. Would this be a wiser application of scarce tax dollars than a rifleshot investment of so much in a single instrument, of speculative urgency, for high energy physics alone? To the lay administrator, the answer could seem obvious. But the lay administrator must not forget that he is an innocent. His views are weighed, along with those of the scientists. The decision to underwrite the project is forthcoming only after a blend of scientific opinion is formed and the need for leadership in a critical area of proficiency is apparent. It may well be the right decision. In any case, the circumstances illustrate the heavy and not always black-or-white choices which are submitted to the Presidency.

But the decisions arising in the area of military weapons development are of an even higher order of difficulty. The adoption of missile-type weapons by the three branches of the services, each branch conscious of its missions, has necessarily led to some profusion of missile systems and incidentally to the identification of status with competing missile programs. To complicate the situation, extensive private contractual interests are involved in the ultimate fate of each of the weapons systems. To bring order out of such a complex, without committing serious error, constitutes one of the most severe modern tests of Presidential management, as well as one of the most thankless.

Presidential aids

An extensive managerial apparatus has evolved gradually to aid the President in dealing with these problems. Within the Department of Defense, responsibility has been focused in a Director of Defense Research and Engineering who advises the President from one tangent while the Joint Chiefs of Staff advise him from another. In the field of space technology, the President is guided by the new National Aeronautics and Space Council, comprising the heads of the military and civilian agencies with an interest in the subject matter. The broader field of general-purpose research and development is gradually being brought into perspective through the Federal Council on Science and Technology. Ranging across the entire military-civilian complex, as the chief agent and adviser of the President, is the Special Assistant to the President for Science and Technology.

But except for the Special Assistant to the President, the role of the consultative bodies is essentially that of agreeing on goals and needs, and defining agency jurisdictions. They do not, and probably could not, due to the well-known limitations of interagency consultative mechanisms, undertake the evaluative process of selection and decision on the particular merits of individual scientific or technological efforts within their limited funding capabilities. Nonetheless, they perform a positive role in the larger structuring of executive management, planning, and co-ordination. The newly created Federal Council on Science and Technology has contributed a great deal, for example, to the over-all understanding within the executive branch of the needs and opportunities for research in the fields of oceanography, basic materials, and meteorology.

The evaluative, or critical, role at the Presidential level is performed in large part by the Bureau of the Budget and the Special Assistant for Science and Technology. In the case of the Bureau, this role is deeply rooted in its institutional character as it has evolved under successive Presidents. The Bureau has developed to a high degree the art of critical examination, and although its skills are predominantly nonscientific, the techniques and processes it employs have been shown to be applicable to the President's decision-making role in science and technology.

Nothing could be more natural in a complex government than to employ the budget process to facilitate decisions for the President. In a way, the budget is a digestive organ with financial limitations on how much can be fed into it. In federal management practice, the budget is the arena of deliberate decision, and the function of budgeting carries with it the corollary of unlimited examination of expenditure requirements.

Budgetary Process

A rare glimpse of the Bureau of the Budget's technique of challenge was afforded in the printed record of hearings held on May 20 and June 17, 1959, by the Preparedness Investigating Subcommittee of the Senate Committee on Armed Services. The subcommittee had before it the Director of the Bureau, and was questioning him on the extent of the Bureau's role in determining the defense budget for fiscal year 1960. In explaining the Bureau's doubts as to the justification for certain expenditures on missile programs, the Director's testimony was as follows: [2]

> Now with respect to the three programs, Atlas, Titan, and Minuteman, our projections indicated that the financial implications over the years 1959 to 1962, inclusive, that is 4 fiscal years, amounted to almost $7 billion for those programs. Now our question was do we get $7 billion worth of value in developing all three of these programs? First of all, there is the protective factor of the SAC Force and the other striking forces in being and the other programs under development, including Polaris.
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> Now Atlas, however, is our earliest available ICBM capability It has presumably less growth potential than Titan, it may be quite vulnerable to surprise attack under certain conditions. Titan at that time was

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[2] Testimony of Maurice H. Stans, Director of the Bureau of the Budget, Hearings, Preparedness Investigating Subcommittee of the Committee on Armed Services, U S. Senate, May 20 and June 17, 1959 (Washington: Government Printing Office), pp. 231–32.

far less developed It was entering a crucial period of flight testing. It appeared to be, from what we could learn, the most formidable of the ballistic missiles that were being planned

On the other hand, it was going to be very expensive, and if it were succeeded promptly by the Minuteman, the question was whether it represented an expenditure that would be justified, and whether our defense would be adequate without it.

As a liquid-propellent system it involved major problems of maintenance and readiness, and while the introduction of storable propellants was feasible, that still involved a considerable amount of research and development.

There was some concern on our part as to whether there was an indication of slippage of the capability dates of Titan, and if that were the case, then it would even narrow the time period between the successful Minuteman and the introduction of the Titan.

On the other hand, Minuteman was in a very early stage of development at the time of our budget consideration. Once it was successfully developed and in large-scale production, it would be the least expensive of the deterrent systems. It would require a minimum level of maintenance. It would be much easier for Minuteman to be hardened and put underground.

However, we were not quite certain that the development of the Minuteman could be successfully accomplished with the level of costs estimated by the Air Force. Or whether or not the timetable could be wholly relied upon. And finally, we had no estimate of the cost of hardening the Minuteman system that we could rely upon.

Now, we took all of those things into consideration and suggested that consideration be given to these three alternatives:

First, phase out the Atlas program as rapidly as possible, relying on manned bombers until the availability of the Titan and the Minuteman. Alternatively, continue the Atlas program as currently planned to provide that initial intercontinental capability, cancel the Titan, and proceed with the Minuteman on a rapid but orderly basis. Or third, continue the Atlas program but with some reduction in the number of squadrons, continue the Titan program with a relatively small number of squadrons but still adequate to provide a heavy striking power, and at the same time proceed with Minuteman on an orderly basis.

Now those are the questions we raised Those are the factors we developed and discussed with the Secretary of Defense. And from there he, with the advice of the scientists that were involved in the programs, and with the President, came to the conclusion that now appears in the budget.

From the sequence of thought described by the Director in the foregoing instance emerges the function of "staffing" of a difficult budgetary problem in the light of the significant factors. This function of the Bureau has come to be valued by all modern Presidents. The final decision is that of the President, but the quality and relentless tenacity of the staff work of the Bureau continue, in the era of "exuberance" in science and technology, to figure prominently in the final outcome.

Science Advisor to the President

Of more recent vintage, but overriding all else in terms of its impact on the changing character of the Presidency, is the role of the Special Assistant for Science and Technology. Born of the incredulous national reaction to the Soviet success in launching the first earth satellite, the office of the Special Assistant has grown in less than two years to a level of status and effectiveness that has little, if any, precedent.

The reasons are diverse. First of all, the traffic of science and technology into the circle of Presidential decision is heavy enough to sustain a full work program for the Special Assistant. Secondly, the President has made it unmistakably clear that the Special Assistant for Science and Technology is his right-hand man in his assigned area, and that he speaks on behalf of the President. Thirdly, the President has assigned a positive responsibility to the Special Assistant for leadership in strengthening United States science and the federal government's part in it. Finally, the first incumbent of the position has demonstrated the personal qualities of vigor, energy, and capacity that the situation has required, while at the same time he has successfully created a modus operandi with the heads of government departments and agencies. In addition, he has had the benefit of diligent work on the part of a Science Advisory Committee, comprised of many of the country's ablest educators and scientists. While the natural tendency of the student of administration is to doubt the workability of unattached special Presidential assistants, the history of the Special Assistant for Science and Technology, on all counts, runs to the contrary.

In a recent address before the National Press Club—itself a significant indication of the stature of the office and its occupant—the departing Special Assistant has given his estimate of the role of a high level adviser to the President on science and technology.

In his remarks,[3] Dr. Killian recognizes the role of the President in motivating effective staff work.

> Let me first express some unabashedly enthusiastic sentiments about my experience. I have derived deep satisfaction from working for the President. I have always found him to be an ***enthusiasm amplifier,*** and always interested in scientific and technological developments. I have never been in to see him, nor has the Science Advisory Committee, without coming out with a sense of lift and renewed zeal—and usually, I might add—some new task or assignment. He is persistent in his desire for information—the rough with the smooth. If his science advisers at any time have not advised, it was not because he failed to afford them the opportunity.

Relationships with Scientists

In Dr. Killian's estimation, the real significance of his assignment has been the "creation of a new and more useful relationship between science and government at the policy-making level." As proof, he declares that government has more readily secured the services of the country's scientists who have responded with enthusiasm. It is a revealing observation, suggestive of the disposition of citizens not only in science but in other occupations to absent themselves from the wedding feast unless and until the summons is delivered by a courier of their own persuasion. Perhaps this is harsh, but the difficulties of the President in securing skilled help are too well known to require elaboration. To the extent that Dr. Killian has moderated the problem with respect to science and technology, the benefits are considerable.

In Dr. Killian's view, moreover, there now exists a larger universe of reciprocal understanding between the scientist and the public official.

> . . . It is now easier for the policy-maker to get the scientific input he needs in his policy-making and to use it with confidence. The generalist and the specialist, the administrator and the scholar, have devised better ways of matching impedances, of working together, each respecting the other's domain and special talent. The scientists have learned to relate themselves better to policy-making without stepping out of their objective role as scientists. The scientists have been able to observe at first hand the way in which many decisions, even those with a large technical content, turn on factors other than technical—social, diplomatic, economic, military, or political. Thus, the policy-makers better understand

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[3] Remarks of Dr. James R. Killian, Jr, Special Assistant to the President for Science and Technology, at the National Press Club, July 7, 1959. Washington, D C.

the role of science in many policy decisions and the fact that these decisions can go wrong if the scientific component is overlooked or wrongly handled There has also been a growing understanding that scientific method and analysis, contrary to popular opinion, do not always yield a single, mathematically exact, incontrovertible answer; that the scientist and engineer, however objective and competent, must sometimes be limited to dusty answers or multiple answers when policy-makers are "hot for certainty."

With these observations, the present writer categorically concurs. Whether the particular issue has arisen in any one of the possible contexts of basic research, applied science, or weapons development, the role of the Special Assistant for Science and Technology has been effective as interpreter and mediator between disagreeing parties, as a mechanism for developing new approaches and needed facts in the framing of policy decisions, and as a helpful device for appraising research needs against the stern facts of budgetary life. Although Dr. Killian has not expressed it in these terms, the elevation of science and technology to the level of the Presidential office has tended to mature the political responsibility of the scientific community. Scientific advice has now been stitched into the fabric of statesmanship, and with the success of the coup, so to speak, the revolutionary fever is abated. Science may still be critical of public policy, but its criticism is increasingly more informed, confident, responsible, and constructive. The great beginnings have been taken to close the long-standing gulf between science and public policy, to the benefit of both, and in particular to the advantage of Presidential responsibility.

SIGNIFICANT QUESTIONS

Nevertheless, there is more to be done. The foundations for effective Presidential action in matters of science and technology have been built well, and show promise of enduring. In this framework, it may now be possible to proceed with the search for solutions to some of the great issues that have been generated by government-in-science. Even though we may have to be content, for a time, with "dusty answers" when we would rather be "hot for certainty," government and science in their new-found nuptials must deal with these nagging questions:

> Is government tending to mobilize science too much for national security reasons, with the result that hardware requirements are overinfluencing the course of scientific innovation?
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> Is there an optimum plateau for government support of science in the United States, apart from the pressures of defense?
>
> Will the national interest require permanent support of science and technological effort at somewhere close to current levels, even if international tensions ease?
>
> When 70 to 80 per cent of university research is dependent upon the flow of federal funds, can we continue to believe that education and science will remain truly independent and free?
>
> Should public policy undertake larger and continuing sponsorship of basic science in the nation's long-term interests, even at the expense of curtailing the present emphasis on spectacular developmental work and outer space exhibitions?

These questions illustrate the background concerns of government in this age of preoccupation with science, technology, education, and survival. They have been on our conscience for a decade, with the solutions still no closer. Because public policy is both fragile and transient, the present contentment which pervades the partnership of science with government cannot justify deferring these fundamental problems merely because our hands are full with today's and tomorrow's problems. A great change, under the pressure of contemporary dangers and imperatives, is being wrought in our time in the relationship among government, science, and higher education; however, it is a change in which the hand of deliberate design should be in greater evidence.

The process of budgeting for science and technology in the context of Presidential responsibility shows encouraging elements of quality, depth, and perspective. It would be a better process than it now is, however, if it had the needed flexibility for the adequate reflection of changing national values. As things now stand, the annual budget is leashed by the costs of national security, debt charges, and a formidable array of "untouchable" benefit and subsidy expenditures which, in the aggregate, reflect yesterday's priorities rather than the new values toward which we might prefer to move. In such a setting of limited Presidential discretion in budgetmaking, new opportunities can be accommodated only in small measure, or through the escapist route of deficit financing. If, for example, fundamental science is not supported with the largesse so religiously urged by its proponents, the reason is not a lack of sympathy on the part of the budgetary authorities but more accurately the inertia of the political process which prevents coming to grips with the task of restating the prime concerns of public policy. To the extent that the budget is treated by the political process as a kind of gift catalogue, rather than an instrument of deliberate national progress toward worthwhile goals, it will not fulfill its potential.

For all that has been accomplished in the structuring of Presidential leadership and responsibility, much more awaits doing in the design of public policies, both coherent and stable, for the fabulous future. Innocents all, we have work to do.

Congress and Research: Experience in Atomic Research and Development

By CLINTON P. ANDERSON and JAMES T. RAMEY

ABSTRACT: Congress has delegated much of its work in relation to atomic research and development to the Joint Congressional Committee on Atomic Energy. The Joint Committee has certain advantages for dealing vigorously with atomic research, including its ability to act for both Houses as a unit, its right to be kept fully informed by the Atomic Energy Commission, and its legislative jurisdiction including annual authorization legislation. The role of the Joint Committee has been evolving from an interest primarily in the weapons program and security to an equal emphasis on peacetime research and development, international co-operation, and declassification. Recently the Joint Committee has tended to stress its informational function through the conduct of various public hearings and dissemination of reports. Of particular importance in recent years has been its policy-making function in atomic power development, basic research facilities, and research in atomic hazards.

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THIS article will be concerned with the interest of Congress in atomic energy research and development, and more particularly with the role of the Joint Committee on Atomic Energy. The editors have wisely permitted the authors to concentrate on a subject with which they are familiar, rather than to attempt to cover the entire breadth of the relationship between Congress and science. It is hoped, however, that this examination may cast some light on the larger problem and may illuminate the question of Congressional leadership vis-à-vis the executive branch. It also may indicate some unique adaptations of governmental institutions to new forces and circumstances.

ESTABLISHMENT AND ORGANIZATION

Congress has applied Woodrow Wilson's maxim—that it does much of its work through committees—in full measure to the Joint Committee on Atomic Energy.

The Joint Committee on Atomic Energy was established by law in 1946 at the same time the Atomic Energy Commission (AEC) was created. It consists of eighteen members, nine from the House of Representatives and nine from the Senate. No more than five from each House may be from the same political party. The 1954 amendment to the basic Atomic Energy Act of 1946 provided that the chairmanship of the Committee should rotate every two years between the House and the Senate.

Congress did not break precedent in establishing a Joint Committee on nuclear energy, as such, in 1946. There were other joint committees, such as on taxation, on printing, and on the economic report. It did break precedent in giving the Joint Committee legislative functions, and also in requiring the AEC and other executive agencies to keep the Joint Committee fully and currently informed. The latter requirement was of immediate importance to the Joint Committee in the early days when its role of "watchdog" over the secret activities of the AEC was paramount. The legislative function of the Joint Committee, as discussed below, has become more important in the last few years with the passage of the 1954 amendments.

FUNCTIONS AND ROLE

Generally speaking, the Joint Committee engages in four types of functions: legislative; investigative; informational; and policy-making. These functions will be outlined below.

Legislative function

The Joint Committee on Atomic Energy, by statute, is the legislative committee for referral of all bills relating to the Atomic Energy Commission or primarily to atomic energy, and may on its own initiative introduce or return bills in either House.

The 1954 amendment and subsequent legislation have had considerable effect on AEC research and development programs. Of particular importance has been Section 261 of the Atomic Energy Act of 1954, as amended in 1957, which requires legislative authorization of appropriations for (1) the construction or improvement of facilities, including research facilities, and (2) the atomic power demonstration program carried on in co-operation with private and publicly owned utilities.

Investigative function

The Joint Committee has not found it necessary to utilize this traditional Congressional tool to any great extent in relation to the research program but on occasion pursues AEC reports to see what is actually being done, and makes studies as discussed below.

Informational function

Under Section 202 of the Atomic Energy Act the Joint Committee is directed to make continuing studies of problems relating to the development, use, and control of atomic energy. This Section specifically directed the Joint Committee to hold hearings during the first sixty days of each session to receive information on the development, growth, and state of the atomic energy industry. The Section also provides that the members of the Joint Committee shall report to their respective bodies as to matters within the jurisdiction of the Joint Committee on Atomic Energy.

The Joint Committee also conducts special hearings of an informational nature on the Commission's basic research programs from time to time. Thus, in 1954 the Joint Committee held hearings on progress in research on agricultural, medical, and other applications of radioisotopes; in 1956 this information was brought up to date, and another series is planned for 1960 or 1961. In 1958 a series of hearings was held on basic research in the physical sciences. In 1957 and again in 1959 extensive hearings were held on the problem of radioactive fallout from nuclear weapons tests, and considerable attention was paid to the status and needs of research in connection with this problem.

These hearings on research and development programs are published and distributed quite broadly by the Joint Committee. For example, 20,000 copies of the 1957 fallout hearings were printed in two large volumes and an index, totalling 2,200 pages. The Joint Committee also publishes, in the form of Committee prints, various reports on hearings, collections of materials prior to hearings, and staff studies.[1]

Although in the early days the Joint Committee tended to give emphasis to the problem of security for information, the Joint Committee, beginning in 1949, came to recognize the positive aspects of a vigorous declassification and publication policy in the field of research. Particularly since 1954 the Joint Committee has urged a greater degree of declassification in such fields as controlled thermonuclear research and reactor development. Sometimes members of the Joint Committee have had to stimulate the executive branch to declassify research results which were being held up for policy rather than security reasons. Thus, the Defense Department's report on its High Altitude (fallout) Sampling Program (HASP) in March 1959 was made public by the Chairman of the Joint Committee rather than the Defense Department. Although it was conceded that no security classification was involved, it was held up with a "confidential" label because it contradicted the predictions of an AEC Commissioner as to the rate of fallout.

Policy-making function

Possibly the most unique function of the Joint Committee has been its policy-making or recommending role in the atomic energy field. This function is explicitly recognized in Section 202 in relation to Congress itself by authorizing the Committee to report its "recommendations" to Congress by "bill or otherwise." However, it remained for the Joint Committee to apply this concept to the executive branch.

In the past and particularly in relation to the military development of nuclear energy, the Committee's policy recommending function has been carried on informally. Classified discussions and executive sessions with top AEC and military officials and the White House have been the principal means of persuasion. The original decision by President Truman to proceed with the vigorous development of the hydrogen bomb was influenced more by Senator Brian McMahon, then Chairman of the Joint Committee, and some of his associates, than by the members of the Atomic Energy Commission. Senator Jackson, in his article in the November 1953 issue of THE ANNALS,[2] pointed out that the corollary effort to increase the AEC raw materials program and to add facilities for the production of the fissionable materials uranium 235 and plutonium was initiated and spurred on by the Joint Committee. The efforts of the Joint Committee in supporting the development of the "Nautilus" nuclear submarine and a nuclear Navy against some determined opposition are now well-known history.

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[1] See Joint Committee print "Membership, Legislation, and Publications of the Joint Committee on Atomic Energy" (Washington, D. C: Government Printing Office, March 1959), for a complete list of Committee hearings and publications

In the past several years, the efforts of the Joint Committee to initiate various policies and projects has not only involved informal methods, but has also taken on more of a public and formal character by means of public hearings and debate, followed by legislative action in some cases. This has been particularly evident in regard to the peaceful uses of atomic energy, but it also has been applied to certain military projects as well.

With regard to so-called "classified projects," the Joint Committee in July 1959 held the first public hearings ever conducted on the status and prospects of the aircraft nuclear propulsion (ANP) program. As a result of Joint Committee efforts this project has been kept alive over the years despite various attempts of the budgetary planners in the Defense Department to bury it. In a report following these hearings, the full Committee endorsed efforts to get a prototype in the air, and recommended that, if the delay continued, Congress might want to consider placing responsibility during this developmental phase in the AEC rather than the Defense Department.

Recent programs sponsored

In 1958 the first public hearings on nuclear propulsion for outer space were held, and efforts by the Joint Committee were continued to accelerate the AEC's nuclear rocket project ("Rover") and auxiliary power sources for satellites program ("SNAP"). In 1959 the first experimental reactors for both projects went into operation successfully. At this juncture the question facing the AEC and Joint Committee would seem to be: How large and at what pace should this nuclear outer space propulsion effort be conducted?

Perhaps the most controversial problem which the Joint Committee has considered in recent years, other than the fallout question, is the matter of atomic power development. Since 1948 and 1949 when the Joint Committee first became interested in the peacetime potential of atomic power, the Committee has sponsored various studies and reports on the need to expand or accelerate the program, and has initiated various efforts to accomplish these objectives.

In 1953 the Joint Committee, under the leadership of Representatives Carl Durham and Sterling Cole, saved the AEC's experimental reactor projects from substantial cuts by the executive branch, and sponsored a five-year experimental reactor program. In 1954, the Joint Committee sponsored the Cole-Hickenlooper bill to amend the 1946 Act to provide, among other things, for greater private participation in atomic power development. In 1956 the Joint

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[2] Henry M. Jackson, "Congress and the Atom," *The Annals,* Vol 290 (November 1953), pp. 76, 78.

Committee passed, without a dissenting vote, the proposed Atomic Power Acceleration Amendment of 1956, which was the Committee's version of the original Gore-Holifield bill. This bill, which was passed by the Senate and narrowly defeated in the House, would have authorized $400 million for the development of prototype demonstration power reactors to be built by the government at AEC sites with the electricity absorbed in connection with operations at the site.

Since 1956, the Joint Committee has focused its efforts in the atomic power development field to adding projects in the annual AEC Authorization Bill. In 1957, among other things, it added a project for a gas-cooled reactor prototype, which was cut down in conference to a design study. In 1958, the Joint Committee authorized the gas-cooled project as well as a plutonium production reactor which would be convertible to electric power production, and various design studies. In 1959, the Committee added three reactor prototype projects to the AEC Authorization Bill, as well as the enlargement of the Shippingport reactor, and several design studies.

Friction with the AEC

The history of the atomic power program has not been entirely harmonious. In 1954 there were differences within the Committee and with the AEC. Again in 1956 and 1957, the AEC and the minority party in Congress opposed the Joint Committee's efforts. In late 1957 and 1958, the Joint Committee and AEC attempted to work out a mutually acceptable compromise. Although much progress was made, AEC came out with a separate statement in June 1958, and a Joint Committee staff report was issued in August 1958. Both statements attempted to provide a compromise between a program composed solely of wholly government owned projects or wholly privately sponsored projects.

During the fall of 1958 the new AEC Chairman, John McCone, appointed an Ad Hoc Committee which provided a report in January 1959 which supported most of the recommendations of the Joint Committee staff report. As of this writing relative peace prevails, with AEC proceeding with projects initiated by the Joint Committee, such as the gas-cooled prototype. The Commission is also assuming a more positive direction of the program, including a statement to undertake the development of a ten-year atomic power program, steps long advocated by the Joint Committee.

Basic research projects

A further example of the Joint Committee's creative efforts is in the field of basic research facilities. The story begins when the Joint Committee's Subcommittee on Research and Development held nine days of public hearings in February of 1958 on the AEC basic research program in the physical sciences.[3] In due course the Subcommittee adopted a unanimous report recommending among other things additional facilities for basic research, and increased operating expenditures in the range of 50 to 150 per cent for research activities.

Following this consideration of basic research, some Committee members became concerned when the AEC authorization request for fiscal year 1959 provided for only two small facility projects totaling about $1,000,000. Under its authority to be kept fully informed, the Committee obtained the information

[3] Some fifty of the nation's leading scientists presented testimony on their methods and results A record of some 800 pages of testimony on the latest data in the fields of chemistry, metallurgy, physics, and controlled thermonuclear reactions was amassed

that the AEC Division of Research had originally recommended fifteen new facility projects totaling approximately $45,000,000. These projects included new and improved particle accelerators, "hot" laboratories, and research reactors. The full Committee passed substantially all of these projects in the AEC Authorization Bill.

Following enactment of the AEC authorization legislation, the Joint Committee Chairman wrote to the Chairman of the House Appropriations Committee calling attention not only to the desirability of providing funds for the basic research projects added by the Joint Committee, but also to the need for funds held up on prior year projects by the Bureau of the Budget. The letter also pointed to the need for additional operating funds. The resulting AEC Appropriation Act for 1959 provided for all funds for prior year projects and half the projects added by the Joint Committee, with the stipulation in the House Appropriations Committee report that cancellations and unused funds on other projects might be used for the remainder.

Upon enactment of the AEC appropriation law for fiscal year 1959, the Joint Committee's Subcommittees on Research and Development and on Legislation announced an investigation of the Commission's research program to discover what bottlenecks, if any, there were to the prompt undertaking of the research and development projects which had been authorized. This was an attempt to forestall the delaying of funds by the Bureau of Budget, an agency which previously had held up some projects for as long as three years and some entirely. Fortunately this red-tape inquiry, together with the new AEC management, produced a fine result, namely that all authorized basic research projects were funded and that work was actually undertaken with reasonable promptness. Such are the somewhat peculiar ways of getting jobs done in the federal government!

The Joint Committee has also been concerned with less understood research needs in the atomic energy field, particularly in relation to radiation hazards. In 1957, for example, following its extensive hearings the Committee urged greater research on the problems of radioactive fallout from weapons tests. In its subsequent report of August 1959, based on hearings held in May, the Committee pointed out that although some progress had been made, the program was still not getting the high-level attention or support which was required. The Committee's reports in 1959 on its extensive hearings on radioactive waste disposal and radiation hazards to employees also called attention to the need for more research on these hazards.[4]

AUTHORITY, METHODS, AND APPROACH

The sources of the Joint Committee's authority in the field of atomic energy are both formal in the statutory sense, and practical in the sense of the realities of government.

Reference has already been made to the Joint Committee's principal statutory sources of authority, namely, acting as a joint unit for both houses of Congress, its right to be currently informed, and its enlarged legislative responsibilities. Also of importance have been the statutory requirements of Joint Committee review of important domestic atomic power development arrangements, as well as international arrangements for co-operation with foreign governments covering the peaceful development of atomic energy and military uses.

From a practical standpoint the success achieved by the Joint Committee over the years has resulted from the continuity in membership of many of its leading members, and its efforts to keep the United States in the forefront of atomic energy development. The tenure of many of its senior members goes back to the original Joint Committee appointments in 1946, and several other members number eight or ten years of service. This is in contrast with the Atomic Energy Commission which currently has four new Commissioners and a new General Manager.

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[4] These radiation hearings, except for employee hazards, have been conducted by the Special Subcommittee on Radiation, Representative Chet Holifield, Chairman

The Joint Committee has exercised its powers vigorously, buttressed by a faith in the importance of atomic energy, and assisted from its inception by an able and devoted staff. The Joint Committee staff comprises a total of approximately twenty employees, including seven or eight professional employees. The Committee and staff utilize extensively the services of the Library of Congress, and the General Accounting Office, organizations which are responsible primarily to Congress. For example, the Library of Congress, at the request of the Joint Committee, prepared in 1956 a report on Soviet manpower and education which foreshadowed the current popular concern with Russian advances. The General Accounting Office, in response to a Joint Committee request, has reviewed the financial and contractual arrangements of each atomic power development arrangement submitted to the Joint Committee for review.

The Joint Committee has also utilized consultants and expert panels to a considerable extent to supplement its own staff and to obtain the benefit of outside advice.[5]

Most informational hearings held by the Joint Committee in the past few years have been planned with the assistance of an ad hoc steering committee of outside experts who advise on the scope and outline of the subject matter of the hearing and on possible expert witnesses. The Joint Committee normally obtains an outside expert consultant to assist on the technical aspects of the hearing and report. In addition, the assistance of the AEC and Defense Department staff is used considerably, particularly in checking factual statements in reports and otherwise obtaining technical information.

Informal approach

Sometimes the Joint Committee has dispensed with formal hearings and used informal seminars where it is believed that less formal proceedings would facilitate frank exchange of information and ideas. This technique was instituted in the development of legislation on power reactor insurance and indemnity, and was carried over into consideration of the atomic power program itself. In these seminars, leaders and technical representatives of the atomic equipment companies, the private and public utilities, engineering and construction companies, and various atomic laboratories, have participated.[6]

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[5] In the development of the Committee's staff report in 1958 on the expanded atomic power program, the following representative panel of reactor and industrial experts participated: Dr Henry DeWolf Smyth, Chairman, Board of Scientific and Engineering Research, Princeton University, Dr Chauncey Starr, President, American Nuclear Society, and Vice President, Atomics International, Mr. Titus LeClair, Manager of Research and Development, Commonwealth Edison Company, Chicago, Illinois; Mr. James Grahl, Director, Atomic Energy Service, American Public Power Association; and Dr. W. H Zinn, President of General Nuclear Engineering Corporation.

[6] Perhaps it was this technique which led an authoritative atomic industry publication to state: "JCAE's assumption of leadership has been quiet, imaginative and efficient, though not unchallenged From the first, the move has been marked by a deliberate effort to consult the industry's executives and the reactoı experts in both industry and government" See Roundup: "Power Shifting Away from

It will be noted that a primary means used by the Joint Committee in keeping informed is by direct contact with technical representatives of the various atomic laboratories and industrial concerns. These Washington discussions are supplemented by field trips to the various domestic atomic energy installations. Perhaps the most interesting field trip of the Committee in the last session of Congress occurred on April 11, 1959, when the full Committee held an official meeting aboard the nuclear submarine "Skipjack" to receive a report from Admiral Rickover on this most advanced type of underwater atomic propulsion. It was during this trip that the Skipjack achieved new records of depth and speed.

Joint Committee members have also participated as Congressional Advisors at a number of international atomic conferences, such as at the Geneva scientific conferences in 1955 and 1958, and the conferences of the International Atomic Energy Agency (IAEA) in 1957, 1958, and 1959.

The Joint Committee's approach or philosophy with respect to nuclear research and development has been that of informed laymen with a vast respect for working scientists and engineers and an abiding faith in the potentials of atomic energy in peacetime.

Encouragement and improvement

The Joint Committee's main tools have been the great reservoir of experience and familiarity with atomic programs described above, together with a considerable amount of common sense and ability to utilize and sift advice and information from many sources. Generally speaking, the Joint Committee's role is not to substitute its judgment for that of the AEC or Defense Department, but rather more to encourage the government to recognize the facts and situations, and sometimes to fill gaps in programs or policies which should be filled. Rarely does the Joint Committee recommend against an AEC request.

It is true that the Joint Committee on occasion does add projects and strongly urge certain policies. This has usually occurred where the AEC or Defense Department has been restricted by budgetary ceilings, or where, as in the atomic power field, AEC was unduly tied to the policy abstraction of private development.[7]

The general philosophy of development followed by the Joint Committee has been one of encouraging orderly improvement in the art. This has caused the Committee to question the construction of atomic reactors for the so-called "Peace Ship" in 1955, and for a Puerto Rico power station in 1957, which would have been virtual carbon copies of existing reactors, rather than improvements. Generally speaking, the Committee has favored diversified and parallel approaches on the important research and development programs, believing that both small improvements and large break-throughs are important in the field of technology. In this connection the Committee has followed a policy of "live and let live" whereby various competing project directors have been encouraged and supported.

The Joint Committee's philosophy in the field of basic research is that facilities and personnel must be augmented in order to keep the United States in a position of leadership in the Western world and in competition with the So-

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AEC on Reactor Plans; Joint Committee, Budget Disputing Leadership," *Nucleonics*, Vol. 16, No 8 (August 1958), p 22

[7] For example, the Joint Committee, in its gap-filling role and based on good technical advice and the British experience, insisted on on AEC sponsoring a gas-cooled prototype reactor. AEC, after initially opposing this type of technology and project, now admits that it is a field which must be explored by government and private development

viets. The Committee in 1956 called attention to the need for an upgrade and an extension of our whole educational system, particularly in the field of mathematics. In 1958, as mentioned earlier, the Joint Committee substantially increased the authorization of facilities for basic research, particularly in the high energy field. The Joint Committee has also called attention to the need for increasing AEC operating expenditures to support the research carried on in the new and improved facilities.

General attitude of Congress

Congress by and large has accepted the legislative recommendation of the Joint Committee with respect to research and development. Occasionally in relation to atomic power development in recent years, the House has refused to agree on federal government construction of prototype demonstration plants. But even here, as a result of Joint Committee and AEC efforts, means of proceeding with such projects have been agreed to and authorized.

OTHER CONGRESSIONAL COMMITTEES

As the peaceful uses of atomic energy expand and the military uses broaden and are better understood, it is inevitable that other Committees of Congress will take more of an interest in atomic energy research and development. For example, the Disarmament Subcommittee of the Senate Foreign Policy Committee has evidenced an interest in research on weapons test detection.[8] The Subcommittee has indicated, however, that it regards the Joint Committee as the "technical" committee in this field, and the Subcommittee is primarily interested in the policy implications of the detection problem.

One of the principal problems in the relationship between Congressional committees is the established practice of not communicating with one another, particularly in regard to classified information or matters discussed in executive session. This practice, in addition to normal jurisdictional interest, tends to encourage committees to find their own facts and information, rather than relying on the work of another committee. Duplication and time wasting of executive agencies and Congressional staff are the result.

IMPORT OF CONGRESSIONAL EXPERIENCE

Perhaps the most pervasive criticism which has been implicit in reviews of the Congressional role in atomic energy research and development, has been that Congress in acting through the Joint Committee majority "has been encroaching persistently on the domain that normally is reserved to the Executive."[9] This criticism has largely been aimed in recent years at the Joint Committee's role in the atomic power development program. It is believed that the review of Joint Committee experience in this article indicates that the Joint Committee from its inception in 1946 has exercised a major policy function in all aspects of the atomic energy field. This role has been carried on without regard to which party had a Committee majority, with most of the Committee actions being unanimous or without dissent.

Whether the results of this active role of Congress in the field of atomic research and development have been good is not for the authors to determine.[10] Suffice it to say that there has been no criticism of the fact that the United States now has additional and better nuclear weapons, more raw materials and fissionable materials, a growing nuclear Navy, and an expanding research and development program for the peacetime atom, all of which have been urged by the Joint Committee and supported by the Congress.

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[8] The House and Senate Committees on Outer Space and Astronautics have an obvious interest in nuclear rocket propulsion The Maritime Committees of the House and Senate also have an evident interest in nuclear ship propulsion, regarding an atomic reactor as just another power plant The Congressional Government Operations Committees have an interest in the organization and management of the atomic enterprise.

[9] See David F. Cavers, "Atomic Power. The Quest For a Program," *The George Washington Law Review*, Volume 27, No 4 (April 1959), p. 470.

From this review of the Congressional experience in atomic energy research, perhaps the following conclusions may be drawn:

(1) The Joint Committee device has worked reasonably well over the thirteen years of its existence.[11] During this period, the Joint Committee's interests have broadened from considerations of the weapons program and security, to research and development on the peaceful uses, international co-operation, and declassification of information. Public hearings and published reports are an important part of its activity.

(2) The Joint Committee experience throughout its existence has shown that Congress can show initiative and exercise leadership even in a complex field.

(3) The Congressional experience in atomic energy does not support the idea of establishing a Joint Committee on Science and Technology to cover all scientific and technical matters coming before Congress. The present scope of activity of the Joint Committee on Atomic Energy is a heavy responsibility, and not much more could be added without overburdening the Committee and staff[12]

A forward look

In conclusion, we believe it might properly be said that what was started as a device to permit a handful of members of the Congress to receive and evaluate the facts concerning a top secret endeavor and stand as guarantors to the Congress and the nation that things were all right has become a useful Congressional instrument at a time when the country requires less secrecy and more free discussion. Perhaps the biggest problem faced by the Joint Committee during this transitional period is that it may lose some of what Representative Mel Price, Chairman of its Subcommittee on Research and Development, calls "its adventurous spirit." But as the pendulum swings away from national secrecy to world co-operation, it is believed that there will be much in the field of research in which a contribution can be made, even in relation to co-operation on the other side of the Iron Curtain.

Plans of the Joint Committee for the current session of Congress include a series of hearings on "The Frontiers of Atomic Energy Research" and a report on a "Review of International Atomic Energy Policies and Programs." It is hoped that these hearings and reports, and others like them, will provide nourishment for the Joint Committee's spirit and will continue to make a contribution by Congress to science and technology.

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[10] For a detailed review and appraisal of the Joint Committee's activities and role up to 1956 see Morgan Thomas, ***Atomic Energy and Congress*** (Ann Arbor· The University of Michigan Press, 1956), pp. 139, 183–84, 229–41.

[11] This is not to say that other devices may not also be of great value. The Senate Committee on Labor and Public Welfare has done a fine job in the field of medical research

[12] Notwithstanding the establishment in 1958 of the House Committee on Science and Aeronautics and the Senate Committee on Aeronautical and Space Sciences, a majority of the committees of both Houses have continued to be concerned with aspects of scientific research and related subjects See Report of the Committee on Government Operations, U. S Senate, "Progress Report on Science Programs of the Federal Government" (Washington, D. C: Government Printing Office, September 1958), pp 58–67

Research Planning and Control in the United States Department of Agriculture: The Experience of an Old and Well-established Research Agency

By Byron T. Shaw

Abstract: Our publicly supported agricultural research has been a joint responsibility of the Department of Agriculture and the state land-grant colleges since 1862. Methods evolved for its planning and control are based on voluntary co-operation between the states and Department agencies, with assistance from farmers, industry, and other groups. This system, nearly as complex and diverse as agriculture itself, has been very successful. Today it involves some 13,000 scientists at hundreds of locations throughout the country. It is financed by nearly $238 million in public funds, about half from the states and half from the federal government. The work concerns all phases of production, marketing, utilization, and consumption of farm and forest products. It is organized generally around specific problems and includes about 14,700 projects. However, more than a fifth of the federal funds support basic studies. Advisory committees, composed of members who represent users of research results, aid extensively in planning. Control procedures, including thorough project review and co-ordination, permit a well-integrated and dynamic program. In the administration of this research, encouragement of creative contributions by individual scientists is a prime consideration.

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FOR nearly a century, agricultural research in the United States has been mainly a public effort, financed and conducted jointly by the federal and state governments. This research is largely responsible for the unparalleled efficiency and productivity of American agriculture.

In no other field of effort has state-federal co-operation been more effective. Our agricultural scientists have been outstandingly successful also in developing close working relations with farmers, farm organizations, marketing associations, industrial concerns, universities and research foundations, and other public and private groups.

The methods gradually evolved for planning and control of this highly complex, co-operative effort are based on the experience and thinking of hundreds of scientists, public administrators, and interested laymen in all sections of the country. They have continually faced the problem of meeting the scientific needs of our rapidly expanding agriculture on relatively limited budgets. Their efforts have resulted in one of the best planned and co-ordinated research programs in the world.

Publicly supported research in agriculture on a nation-wide scale began in this country in 1862, following enactment of two federal statutes creating the Department of Agriculture and the state land-grant colleges.

Subsequent acts passed by Congress established the state experiment stations and provided them with federal-grant funds, extended the scope of scientific work by the Department, and strengthened the bonds between federal-state research in agriculture and agricultural education.

Public institutions developed as a result of these laws have provided the leadership for our agricultural revolution.

ROAD TO ABUNDANCE

Farming technology in the 1860's had progressed little beyond that of Colonial days. Our fast expanding population was fed chiefly by more and more farmers working more and more acres. This trend, accompanied by a slow rise in farming efficiency, continued until the end of World War I.

Productive farm acreage tripled between 1860 and 1920, reaching a total of some 500 million acres cropland equivalent. Although important shifts in land use have since occurred, this total amount of cropland remains the basis of our agriculture today. Farm employment rose to an all-time peak of 13.6 million during the years 1910–16 and then began a steady decline.

Since 1920, advances in technology, rather than additional land and manpower, have been the key to our agricultural progress.

From 1920 to 1940, production of all farm products rose about one-fifth, while cropland stayed the same and farm employment dropped almost a fifth. But this was only the beginning.

In the twenty years since 1940 we have raised our total agricultural output by more than half. Farm employment has fallen by one-third. Farm output per man-hour has doubled.

Besides giving us a greater variety of higher quality farm products, agricultural research has developed more efficient methods for marketing and distributing these products to consumers. It has improved the diets of all our people, lightened the work of homemakers, and contributed in other ways to better family living.

The dark side of this picture of progress is well known Farm production has been running ahead of demand The burden of surpluses is felt by the entire economy. Prices received by farmers have declined relative to prices of goods they must buy. As a result they have not shared equally with other segments of the population in the country's general prosperity.

THE RESEARCH JOB TODAY

Most of the phenomenal increase in farm output achieved in the last twenty years has been vitally necessary to meet the needs of a population that has grown by nearly 48 million people since 1940. Research owes no apology for its contribution to our bountiful harvests. But it does recognize a responsibility for doing something about the surpluses and the "cost-price squeeze" on farmers.

We are endeavoring to find new and wider uses for farm products, especially as industrial raw materials. We are seeking ways to help farmers make needed adjustments in production—away from commodities in surplus, and toward commodities for which demand is expanding, or can be expanded.

At the same time, we must continue to increase the quality of farm products and lower the cost of their production, further improve the efficiency of farm-product distribution, find ways to prevent soil deterioration and loss, and halt the waste of our water resources.

Looking ahead, we can expect a doubling of our present population in the next fifty years. For our people then to be as well fed as they should be, we must at least double our present crop output and more than double production of animal products. The job will have to be done on about the same acreage we have now, and with probably still fewer farm workers.

Federal-state responsibilities

In doing its share of this research job, the Department of Agriculture directs most of its efforts toward problems of regional or national significance. The state agricultural experiment stations are free to investigate any problem of interest to the people of their states, but on matters of regional or national interest they usually work in co-operation with other states and the Department.

These stations conduct about three-fifths of the total federal-state research program. About 78 per cent of their work is currently supported by state funds and the remainder by federal grants.

Both state and federal research administrators endeavor to maintain voluntary working relationships that will avoid unnecessary duplication in their research and yet provide scientific answers to the most important problems, whether they are local, state, regional, or national in scope.

The Department and the experiment stations co-operate formally on more than half their research and informally on most of the rest. We are proud of this system and the way it operates. No other country has anything like it.

Size of the effort

The Department employs about 4,800 scientists, including specialists in practically all of the biological sciences, most of the physical sciences, and many of the social sciences. They work at more than 300 locations in all the states. The facilities at two-thirds of these locations are state owned, and the remainder are in federal establishments.

The state experiment stations have about 8,400 scientists, some of whom are engaged part time in teaching or extension work and part time in research. They work side by side with Department people at many of the research stations in each state.

Public funds appropriated for federal-state agricultural research have doubled since 1953. For the current fiscal year they total about $238 million. A little over half—$124 million—is provided by the federal government and the remainder by the states.

The federal portion includes about $92 million to be spent by Department of Agriculture scientists and $32 million in federal grants to the state experiment stations. The state portion includes some funds provided to the experiment stations from private sources.

Since 1946, the Department has had authority from Congress to contract with outside groups for research, under its supervision, that can be done more rapidly, more effectively, or at less cost by other institutions than in its own facilities. At present, we have more than 200 such contracts in force.

Under the Agricultural Trade, Development, and Assistance Act (Public Law 480), we recently began developing an extensive program of contract and grant research in foreign countries, financed by local currencies from the sale abroad of United States surplus farm commodities.

Industrial research in agriculture has also nearly doubled in the last half dozen years. It is supported today by an amount roughly equivalent to that for state-federal research. The work by industry is heavily concentrated in relatively few fields, mainly in processing and marketing, though substantial funds are spent also on agricultural chemicals and biologics and on farm equipment.

DEPARTMENT ORGANIZATION

During most of its history, research in the Department was a function of several tightly organized bureaus. Each bureau provided its own administrative services and presented its own budget estimates to Congress. Most of the bureaus were responsible not only for research, but also for certain regulatory activities that required research support, such as federal meat inspection and the control or eradication of livestock and crop pests and diseases.

Since 1942 a series of changes has been made in the Department's research organization. These changes, reflecting the increased complexity of both agriculture and agricultural science, provided for the redirection and closer co-ordination of the Department's research efforts to meet changing national needs. In the process the old bureau structure has been abolished.

But the general principle of conducting research along with other activities in multifunctional institutions has been maintained, both in the Department and in the state colleges. The Agricultural Research Service and other Department agencies engaged in research continue to have regulatory and other responsibilities. Although similar regulatory activities in the states are largely a function of the state departments of agriculture, the land-grant colleges conduct teaching and extension work in addition to research.

The administrator of the Agricultural Research Service is delegated responsibility for the co-ordination of all the Department's research and for the administration of federal-grant funds to the state experiment stations.

Research activities of the Agricultural Research Service cover four broad areas:

1) Seven divisions, under a Deputy Administrator for Farm Research, work in the fields of livestock, poultry, and dairy husbandry; animal diseases and parasites; entomology; soil and water conservation; agricultural engineering; crops production; and farm economics.

2) Four divisions, under a Deputy Administrator for Utilization Research and Development, are concerned with expanding industrial and other uses for farm products.

3) Two divisions, under a Deputy Administrator for Experiment Stations, administer federal-grant funds to the states

and direct our territorial experiment stations.

4) The Institute of Home Economics, headed by a Director, conducts research in human nutrition, household economics, and clothing and housing.

Four other Department agencies are responsible for important additional phases of the total program. These include the marketing research and statistical functions of the Agricultural Marketing Service; research on forestry and forest products by the Forest Service; research aid to farmer co-operatives by the Farmer Co-operative Service; and foreign market development work by the Foreign Agricultural Service.

ADMINISTRATIVE METHODS

The diversity of American agriculture, the growing interdependence of all science, and the complexity of a research organization composed of some fifty autonomous state experiment stations, plus nation-wide activities by several Department agencies, make the planning and control of our state-federal research program a formidable undertaking.

Administrators of this research must insure that the important problems receive adequate attention; that shifts in economic and other conditions are properly reflected in the research program; that unnecessary duplication of work is avoided; and that the benefits expected from particular lines of work justify the effort devoted to them.

Choosing the best available ideas for incorporation in any research program is of course important. It is equally important to stimulate a flow of good ideas from which a choice can be made.

Accordingly, in our federal-state system we encourage both the inflow of ideas from outside and the generation and interchange of ideas at all points within the system.

Advisory committees

We make a continuing effort, formally and informally, to solicit comments on our research program from those best acquainted with the problems we are trying to solve. Valuable mechanisms for this purpose are the Department's Agricultural Research Policy Committee and the twenty-five advisory committees established under provisions of the Research and Marketing Act of 1946. Members of these committees represent the major segments of United States agriculture and the consuming public.

The Agricultural Research Policy Committee meets quarterly and advises the Department on policy matters and broad adjustments needed to maintain a dynamic research program.

Seventeen of the advisory committees are concerned with particular farm commodities, and the other eight offer counsel in various functional areas, such as food and nutrition, transportation, and economics. They meet annually to review current research and recommend adjustments, including termination of existing projects, expansion of current work, and initiation of new work.

The state experiment stations seek advice concerning their own research from similar advisory committees, composed of representatives of farmers, consumers, and other groups having agricultural interests in the various states.

The project system

Scientists at all levels of the federal-state research organization play an important part in the initiation and planning of research. They recognize problem areas where new facts are needed, and they can be counted upon to consider the usefulness and originality of projects they submit to their supervisors. The administrator's job is to determine in each case whether a proposed project will contribute to solution of the most urgent research problems and fit into a well-balanced total program of research.

The administrator of the Agricultural Research Service provides for the examination and analysis of all Department research activities, current and contemplated; for review and approval of all proposed projects before they are initiated; for advice and consultation with agency heads on the planning of research; and for submission of reports and recommendations to the Secretary of Agriculture. He is assisted in this work by a Central Project Office and an Agricultural Research Council.

The Central Project Office is a control center in the review and approval of research proposals. It maintains records on about 2,800 Department line projects. Each proposed project is examined in this office in relation to the existing program and referred for comment to all agencies of the Department doing related work, including subject-matter specialists in the State Experiment Stations Division of the Agricultural Research Service, who consider it in relation to research at the state stations.

The Agricultural Research Council includes the deputy administrators for research in the Agricultural Research Service and in the Agricultural Marketing Service, the director of the Institute of Home Economics, the assistant chief for research of the Forest Service, and the administrators of the Foreign Agricultural Service and the Farmer Cooperative Service. It provides a forum for discussion of research problems of Department-wide concern.

Federal-state research

Relations between the states and the Department involve both the administration of federal-grant funds and a great volume of co-operative research which is formally documented by co-operative agreements and memoranda of understanding.

The Department's role in the federal-grant research program of the state experiment stations is primarily one of service. We are charged with seeing that the funds are spent as intended by Congress, but we have a further responsibility for giving technical assistance. This includes comprehensive reviews of federal-grant research, participation in planning of regional research, and co-ordination of research effort among the states as well as between the states and the Department.

Before research projects using federal-grant funds can be initiated by a state station, they are reviewed in the State Experiment Stations Division. Division specialists make annual visits to each station to discuss the progress of the research with the scientists doing the work. At most of the stations, this annual review covers all state projects and is not limited to those receiving support from federal-grant funds.

The Division keeps detailed records on about 5,900 projects at the state experiment stations financed wholly or in part by federal funds. In addition, information is maintained on some 6,000 projects financed entirely by state funds.

These records, along with those on federal projects kept in the Central Project Office, provide background information on current work in progress against which all proposed new projects to be supported by federal funds are examined. This is the key to effective co-ordination and prevention of unnecessary duplication in the total effort.

In co-operative research on all problems of mutual interest, state and federal workers decide jointly what portion of the work each will undertake. To aid in co-operative planning, we make use of various devices that have proved useful over the years.

They include standing technical committees of state and federal scientists in major subject-matter areas, such as cereal crops, beef-cattle breeding, and other areas of general interest and importance. From time to time we also establish special research panels to plan an attack on emergency problems, such as new plant or animal diseases.

An important provision of the law on federal-grant funds is the authorization for co-operative regional research. It encourages joint planning and conduct of work on problems of broad regional significance by several state experiment stations and, frequently, one or more federal agencies. About 12 per cent of the research funds available to the state stations is devoted to regional projects. We maintain a variety of federal-state co-operative arrangements for this work.

Co-operation with industry

For the most part co-operation with industry is less formalized than state-federal co-operation. However, industrial research grants, endowments, and fellowships play an important part in co-operative industry-state research. Industry also co-operates extensively with the Department. Facilities, materials, and services made available by private concerns aid many phases of both federal and state research.

RESEARCH ENVIRONMENT

In developing our methods for the planning and control of research, we keep continually in mind the fact that successful research depends mainly on the ability of individual scientists. To generate those new ideas on which scientific progress depends, they must be talented and well-trained people, and they must have the freedom to use their imaginations.

Co-ordination, integration, and supervision of research are necessary. But we have to face the fact that a large element of self-direction is essential for productive work by scientists. A research environment favorable to creative accomplishment by individual specialists must be provided.

A scientist should be able to conduct his research in an atmosphere of free inquiry, to publish his results freely, and to take advantage of free discussion with his colleagues.

One powerful incentive in scientific work is the opportunity to gain prestige and recognition. A liberal policy on attendance at scientific meetings and the holding of office in scientific organizations is therefore important.

In publishing their research results, scientists should be able to state their findings in their own words. Excessive review or editing of scientific papers, unrealistic security classifications, and too-rigid style requirements should be avoided. Accuracy of published papers is vital, but within the limits of accuracy and reasonable clarity, the individuality of the scientist as expressed in his reports should be maintained.

Pay is of course important to scientists, even if it is not the most powerful incentive for their work. We are making progress toward higher salaries for research work and better recognition of the value of research genius as distinguished from administrative ability.

The number of positions in the Agricultural Research Service in which senior researchers can work without administrative responsibilities has been increased. And our scientists now have opportunities in several positions to receive compensation fully equal to that of the agency's top administrators while continuing to pursue individual research.

Team research

We find that productive individual effort and the team approach can be effectively combined in a good research environment. Team research, though sometimes condemned as regimented, can help achieve co-ordinated effort, economy, and desired results.

Our co-operative relationships with the state experiment stations, as well as the diversity of our own work, call for a great variety of arrangements of this type. More and more team research is being done, because the nature of the problems to be solved demands it and because we have learned how to do it successfully.

BASIC RESEARCH

Our traditional "problem approach" in agricultural research has resulted in a strong emphasis on work aimed at finding practical answers to immediate questions. Such an emphasis is not inappropriate in a research effort supported by the public.

But many of our problems today, as well as those likely to arise in the future, call for more basic research. It is the only means we have for discovering the new scientific laws and principles that will enable us to deal more effectively with all problems.

Over the past dozen years, the proportion of the total effort in public agricultural research devoted to basic studies has risen from about 7 per cent to better than 22 per cent. In the Agricultural Research Service, we look forward eventually to putting at least 50 cents of every research dollar into the quest for new basic discoveries.

Most of our basic research will continue to be associated with problem solving. But we are increasing our efforts also in pioneering research—fundamental studies aimed at advancing basic knowledge without direct concern with practical problems.

We have established in the various divisions of the Agricultural Research Service during the past two years fifteen Pioneering Research Groups. Scientists chosen for this pioneering work have none of the supervisory or administrative duties usual in government positions.

They are free to seek answers to some of the basic mysteries of plant nutrition, physiology and virology, plant fibers and enzymes, seed proteins, the allergens of agricultural products, animal genetics, animal proteins, blood antigens, cellular metabolism, insect physiology and pathology, the chemistry of micro-organisms, and the economics of interfirm integration in farming.

Our pioneering researchers labor at the very frontier of knowledge. Many of our other scientists also are helping to advance that frontier. Their job, like that of the Western pioneers in the 1860's, calls for individual initiative. It puts a premium on co-operative enterprise. It demands a free environment.

The Department's experience of nearly a century teaches that in all our research planning and control we should strive to maintain the freedom of research, encourage co-operation, and stimulate the initiative of individual scientists.

Research Planning and Program Development in the National Institutes of Health: The Experience of a Relatively New and Growing Agency

By Herbert H. Rosenberg

ABSTRACT: Research planning and program development in the National Institutes of Health (NIH) have emerged over the past decade in a context of rising national interest, steadily expanding funds, shifting program emphasis, and constantly broadening scope of scientific inquiry. NIH basic philosophy seeks to maximize creative potential and regards research planning as a totality encompassing the limiting resources of manpower and facilities as well as conduct of research itself. Planning has evolved as a continuous, flexible, adaptive process in which administrators and scientists possess considerable discretion within a framework of goals and priorities established by Congressional action. It represents a balance between the spontaneous interests of individual scientists and organized effort designed to meet needs and opportunities vital to the nation's health. Responsibility for over-all planning has grown as expenditures have increased, program interests have enlarged, and NIH has become a more significant factor in support of university research and training of scientific manpower. The nature of the process requires further adaptation to deal more effectively with the problems of the future such as total impact of NIH programs upon an institution as a whole, strengthening the nation's research structure, and administering a co-operative research program on a world-wide scale.

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IN a growing agency,[1] the scope of planning constantly enlarges to meet program needs and opportunities in consonance with broadening interests and expanding responsibilities. As this development takes place in a research organization, new perspectives must be reconciled with the goal of maximizing the creative potential of scientific talent. The National Institutes of Health (NIH) confronts these problems as it grows within a context of rising national interest, steadily expanding funds, shifting program emphasis, and constantly broadening scope of scientific inquiry.

The past decade has crystallized the national interest in medical research. Extending beyond the support of science as an end in itself, the national interest recognizes that medical research deals directly with the ultimate resources of the nation—the lives and health of the people. It emphasizes the contribution of medical research to economic capability. Better health and longer lives mean a larger, more productive labor force and reduction of individual and social costs of illness. Finally, medical research serves as a hopeful avenue toward peaceful coexistence between the free world and communist ideology.

The rapid but steady growth of the National Institutes of Health represents the development of a large-scale medical research program to promote the national interest. Facilitated by bipartisan support, NIH research appropriations have increased 20 to 30 per cent annually (Figure 1). This evenly paced growth pattern has expanded research funds more than thirty-fold since 1947, from $8 million to $285 million.

Concomitantly, program emphasis shifted from the infectious to the chronic and degenerative diseases. The scope of medical research broadened to em-

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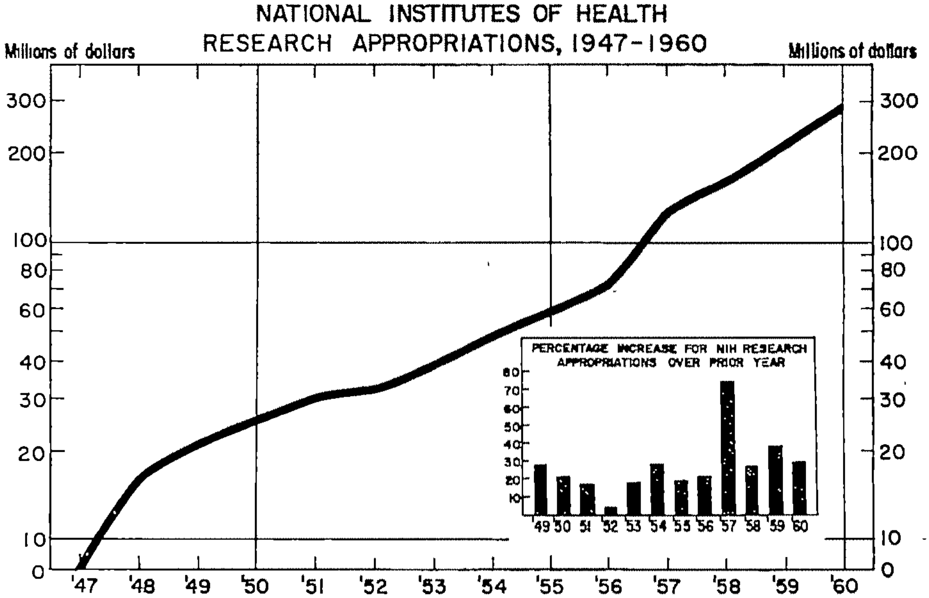
[1] The views expressed are those of the author. They do not necessarily reflect the official views and positions of the National Institutes of Health, the Public Health Service, or the Department of Health, Education, and Welfare.



Source: National Institutes of Health

FIG. 1.

brace the creative energies of men from all fields of science. Increasingly, the study of biological processes utilizes the concepts and methods of the physical sciences. Behavioral scientists also devote their talents to medical research, especially as it touches mental health and related problems of individual and social well-being.

A "NEW" FEDERAL AGENCY EMERGES

Within this milieu, the National Institutes of Health has emerged as the federal agency with primary responsibility for medical and health related research. Beginning with a brief sketch of historical, organizational, and philosophical influences, this article describes NIH experience with research planning and program development in the light of past and present approaches and future problems.

Historical background

NIH is not exactly a "new" agency; its research tradition is deeply rooted in the Public Health Service established in 1798. The Hygienic Laboratory, opened by Dr. Joseph Kinyoun in 1887, preceded NIH as the Service's major research activity. This one-room laboratory expanded into a research organization with separate divisions of chemistry, bacteriology, pathology, zoology, and pharmacology. Its research interests broadened further in 1912 when the Public Health Service was specifically empowered to "study and investigate the disease of man." In 1930, the National Institute of Health was created and took over the functions of the Hygienic Laboratory; it moved from its five-acre site near 25th and E Streets, N.W., in Washington to the 305-acre campus at Bethesda eight years later.

New knowledge resulting from research coupled with public health measures drastically reduced the death toll from infectious diseases between 1900 and 1930. The challenge of the future loomed in the major killers—cancer and heart disease—and the cripplers such as mental illness, metabolic and neurological diseases.

Recognizing that research offered the best hope of combating such diseases, the Congress established the National Cancer Institute in 1937. The statute, which served as a prototype for the creation of additional Institutes for other disease categories, empowered the Cancer Institute to conduct and coordinate research, make grants, and provide fellowships and training, and created a National Advisory Cancer Council. These actions marked a break with the past; they gave the National Institutes of Health a key role in the stimulation and support of research aimed at improving the health of the people.

Structure and functions

The National Institutes of Health is one of several bureaus of the Public Health Service within the Department of Health, Education, and Welfare. Its research programs are administered by seven National Institutes which cover the following categories: Cancer, Heart, Mental Health, Arthritis and Metabolic Diseases, Neurological Diseases and Blindness, Allergy and Infectious Diseases, and Dental Research. Each Institute receives a separate appropriation; each conducts intramural and extramural research and training programs. In addition, the Division of General Medical Sciences supports research and training not directly related to any specific disease and operates the Center for Aging Research.

The Director of NIH provides executive leadership, directs programs and co-ordinates the Institutes. In discharging this responsibility he is assisted by a Deputy Director, two Associate Directors—one for intramural research and one for training programs

—and three senior staff members in the areas of research planning, administration, and information.

This summary oversimplifies a multifaceted structure. In reality, NIH operates simultaneously in four different organizational contexts: (1) A bureau functioning as the principal research arm of the Public Health Service; (2) a national research institution at Bethesda carrying out what is perhaps the world's most comprehensive medical research program; (3) a major national agency whose policies and programs in support of research through grants and contracts have a far-reaching impact upon the scientific and academic communities. For example, during the past year NIH supported more than $140 million in research grants covering about 10,000 projects in nearly 1,000 institutions; and (4) the federal agency responsible for continuing appraisal of research needs related to human health and disease as well as opportunities and resources required to meet those needs.

While the character and scope of research planning differ markedly for each of these organizational contexts, the basic philosophy remains constant.

Basic philosophy

Research planning at NIH seeks to maximize the creative potential of the limited supply of high-quality scientists. This basic philosophy encourages the scientist to follow his own curiosity in search of truth. It assumes that support of men and ideas in an environment conducive to creative effort enhances the probability of significant findings. This view has shaped the development of intramural research. In the extramural grant program, it has fostered terms and conditions of support that nourish free inquiry.

NIH also regards research planning as a totality which encompasses the limiting resources of manpower and facilities as well as the conduct of research itself. This philosophy calls for a continuing appraisal of resources and requirements and the development of programs to: (1) assure an adequate supply and improve quality of scientists in all fields which are basic to medical research and (2) help provide the costly, complex research facilities required to advance the frontiers of knowledge.

WHO PLANS WHAT AND HOW

Planning at NIH has evolved as a continuous, flexible, adaptive process. It has to be because plans are influenced by the fusion of so many forces: Congressional interest and understanding, executive branch policies, values and views of the scientific community, attitudes and actions of lay groups concerned with specific diseases, recommendations of advisory groups, the vision and strength of NIH staff at all levels, and the leadership of three men who share administrative and political responsibility—the Director, NIH; the Surgeon General, Public Health Service; and the Secretary, Department of Health, Education, and Welfare. As a consequence, NIH research planning has developed as an integral part of the politico-administrative life of a growing organization, not as an isolated, autonomous study process.

In large measure, the Congress sets the goals by creating separate Institutes which focus upon specific diseases or health problems; it establishes priorities and allocates resources through the annual appropriation and accompanying reports. Within this framework, administrators and scientists possess considerable latitude for planning and program development. The problems they face vary substantially, however, for each of four major areas: intramural research, extramural grant support, target research, and over-all planning for the future.

Intramural research

The problems which NIH has experienced in its intramural research programs prevail in most large-scale research organizations.[2] First, NIH has sought to attract creative scientists, to give them adequate assistance and research facilities, and to provide an environment conducive to productive effort. The second set of problems involves the translation of statutory missions such as "to conduct . . . research . . . relating to the cause, prevention, and methods of diagnosis and treatment of heart disease . . ." into specific programs. Third and thorniest is the reconciliation between the freedom essential for productive inquiry and the planning, direction, and control which any large-scale organization must exercise to assure responsible performance. Problems such as these are not "solved"; they are worked at and lived with.

The Director and his staff play a role with respect to the level of effort, broad areas of investigation, and general direction. The degree to which the Director's office influences the substance of research fluctuates among institutes and over time.

Scientists determine the substance of the intramural research program within the constraints imposed by available funds, talent, space, and time. These decisions hinge upon judgments of research needs and opportunities, state of knowledge, and particularly the scientific interests and competence of the staff on hand.

The broad scope of "heart disease," "cancer," "mental health," and other fields confers ample discretion for the development of specific research programs and projects. Moreover, because history has demonstrated that fundamental inquiry accelerates understanding of basic disease processes, research is not narrowly confined to the diseases associated with each Institute.

At each successive layer, the scientific director, laboratory chief, section head, and so on, allocate resources and determine the degree of freedom for those beneath them. Freedom is not inherent in the individual's status as a scientist but is an earned right. NIH policy is to provide the degree of freedom consonant with the scientist's performance and promise. This evaluation is crucial because men, not projects, govern research productivity.

While informal peer judgments influence this appraisal, regularly scheduled promotion reviews furnish a more systematic evaluation of research accomplishments. In addition, an annual review of intramural research begins with the individual investigator, progresses through successive levels, and culminates in a document which serves as a springboard for the discussion of future and continuing research plans.

In addition to the assessment of men, another problem plagues most research organizations: How to identify changing needs, new opportunities, and shifts in scientific promise and how to modify research content accordingly? To provide a fresh and uncommitted appraisal, NIH utilizes boards of scientific counselors, drawn from the nation's top scientists. These groups review the intramural research program semiannually and make their recommendations for consideration by the Scientific and Institute Directors and the Director of NIH.

Extramural research

The National Institutes of Health invests eighty cents of its research dol-

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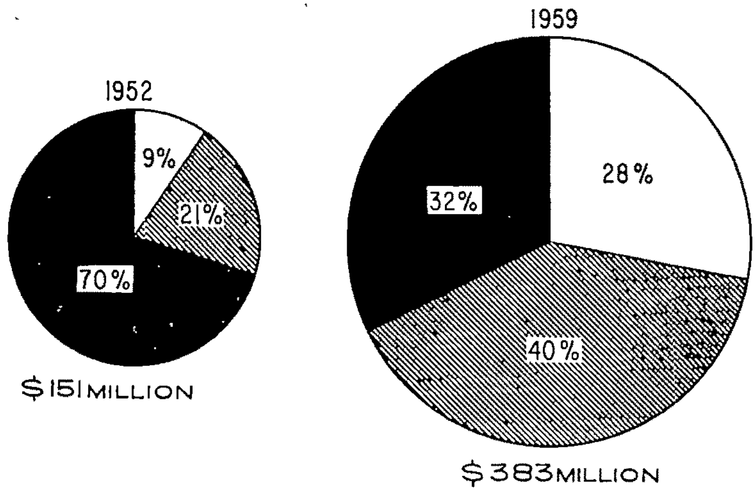
[2] For an excellent, comprehensive discussion of NIH experience which is as pertinent today as when it was written, see W H Sebrell and C. V. Kidd, "Administration of Research in the National Institutes of Health," *Scientific Monthly*, Vol. LXXIV, No 3 (March 1952)

TRENDS IN FEDERAL SUPPORT OF RESEARCH AT ACADEMIC INSTITUTIONS, 1952-1959



FEDERAL FUNDS FOR SUPPORT OF UNIVERSITY RESEARCH (millions of dollars)

CODE	AGENCY	AMOUNT 1952	AMOUNT 1959	PERCENT 1952	PERCENT 1959
	TOTAL	$151	$383	100	100
□	NIH	14	106	9	28
■	Dept. of Defense	106	124	70	32
▨	Other Federal Agencies	31	153	21	40

Source: Derived from National Science Foundation Federal Funds for Science, I and VIII

FIG. 2.

lar in extramural research. Moreover, this investment has increased to the point where it now comprises a significant proportion of all federal research expenditures in academic institutions (Figure 2).

Despite this rapid but steady growth, the fundamental principles adopted in 1945–1946, when NIH agreed to take over the active contracts of the Office of Science Research and Development's Medical Research Committee, continue to govern extramural research planning and program development. These principles are: (1) support of the best men and most promising ideas will produce the new knowledge required to combat disease and improve health; (2) the best men will seek support only if NIH gains and holds the respect of the scientific community; (3) support for the best men and ideas is most likely to emerge from open competition based upon scientific merit; (4) the judgment of one's scientific peers provides the wisest and fairest method for evaluating scientific merit, promise, and feasibility; and (5) terms and conditions that assure reasonable stability of support and foster freedom and flexibility for the investigator will facilitate productive inquiry.

The first step toward translating principles into action occurred in 1946 with the establishment of study sections organized around scientific disciplines, such as biochemistry and physiology, and problem areas, such as mental health and radiation. Composed of eminent scientists drawn from the scientific community, these study sections review all applications for research support in their particular discipline or problem area and assign priorities based upon scientific merit, promise, and feasibility.

The next level of review consists of eight twelve-member National Advisory Councils, each composed of six specialists in the appropriate medical field and six outstanding experts in fundamental science and public affairs. No grant may be made unless recommended for approval by a Council. The Councils also deliberate and advise on broad issues and policy questions.

Thus, with some significant exceptions, the substantive content of the extramural grant program is *not* planned. It emerges from the interaction of the research interests of qualified investigators and peer evaluation of scientific merit. Clearly, the scientific community plays the dominant role in shaping the content of the extramural grant program.

Target research

There are, however, significant exceptions where conscious planning supersedes purposeful nonplanning. The stimulus for conscious planning and direction may come from the scientific community, from NIH staff, from Congress, from health groups, and, most likely, from a combination of these sources. Conscious planning is essential to:

1. Nourish new fields of science Study sections survey the status of research in their field to ascertain current emphasis and future needs. Biophysics is an excellent case in point. The survey of this nascent field concerned with the relationship and interaction of matter and energy in life forms took several years, involved scientific leaders throughout the world, and resulted in specific recommendations to Congress, study sections, Councils, and NIH staff, and culminated in programs to stimulate research and expand the critical resource, trained manpower

2. Identify and remedy gap areas. As the proportion of older persons in the population rises, the need for more intensive study of the physical, biological, and social aspects of the aging process becomes more acute. While several of the Institutes had long supported some research on various aspects of gerontology, NIH staff and Advisory Councils recognized the need for a concentrated, comprehensive approach., To provide a focus for such an approach, the Center for Aging Research was established in 1956. The program has grown since then, particularly through grants to non-federal research centers for long-term multidisciplinary studies.

At the opposite extreme of the life cycle, we find that more research is badly needed on the problems of perinatal morbidity and mortality. Because even a slight reduction in the mortality rate would yield substantial individual and social benefits. NIH has planned and initiated a nation-wide collaborative study of these problems.

3. Accelerate break-throughs The evidence that virus can cause cancer in animals has been well established and scientists have developed vaccines that protect mice and chickens against virus-induced malignant tumors Because scientists feel that the present state of knowledge seems ripe for a break-through, the Congress earmarked an additional $4 million for research to determine whether viruses cause *human* tumors.

4. Evaluate intensively new methods of diagnosis or treatment. The primary illustration is the cancer chemotherapy contract program created by Congressional directive in the 1957 appropriation. It represents a highly organized program for screening and testing chemical therapeutic agents. It is altogether possible, however, that the scientists involved in this intensive effort may

stumble upon new knowledge of a fundamental character outside the target area.

In summary, because of these widely divergent approaches to the support of extramural research, the program represents a balance between: (1) the spontaneous interests of individual scientists and (2) organized effort designed to meet specific needs and opportunities considered sufficiently vital to the nation's health to warrant central planning and direction.

Stability versus flexibility

When research planning focuses upon policy rather than substantive content, the scientific community participates but does not control. NIH staff consults with study sections and Councils in planning for terms and conditions that nourish free, creative inquiry. Policies have encouraged grants for broader areas of investigation and for longer periods, three to seven years, thereby promoting greater stability and flexibility for the investigator.

These policies commit a substantial share of each year's program for a minimum of two years ahead. Rising research costs offset the apparent flexibility achieved by terminations and continuing evaluation. Consequently, program development must take place at the margin in terms of support for new men and ideas.

This fact is often overlooked in headlines publicizing NIH appropriations. However, NIH must plan for annual increments adequate to support a reasonable proportion of *new* men and ideas if the nation is to benefit from the constantly expanding supply of scientists seeking support for medical research.

Over-all planning

As research expenditures have expanded and program interests have broadened, NIH responsibilities for over-all planning have grown with respect to the nation's total medical research effort. Ideally, over-all planning seeks to: (1) achieve desirable balances among competing objectives; (2) formulate long-range plans for future growth; (3) develop policies which will facilitate the attainment of these balances and plans; and (4) adjust and modify programs in consonance with this over-all framework, taking into account shifting scientific opportunities and health needs, changing economic and social conditions and pressures, and political realities.

In its over-all planning, NIH has become increasingly conscious of the dilemmas of balance. One of the early problems was to strike a desirable equilibrium between the intramural and extramural research programs. Another problem was to reconcile freedom for the investigator with specific disease missions. What relative emphasis should be given to today's research, construction of tomorrow's facilities, and training research scientists for the decade ahead? How should research funds be allocated between the advancement of new knowledge and the rapid translation of existing knowledge into practical application? How can NIH achieve a better balance between its research objectives and its broader responsibility for strengthening the nation's institutions dedicated to education, research, and medical care?

Questions such as these necessitate planning and continuous re-evaluation The NIH experience has divined no magic solution to these complicated issues. It has, however, demonstrated the need for better long-range planning to provide a perspective for viewing these issues within a framework of probable future developments.

Long-range plans for the future growth of medical research rest upon assumptions which take into account a host of variables such as population and economic growth, the prospect of substantial scientific achievements, the enlarging horizon of investigation and the penetrating depth of inquiry, the long lead-time required for training of manpower and construction of facilities, and the growing internationalization of the scientific community. The development of such assumptions requires a steady flow of relevant data and systematic analysis of all factors bearing upon the nation's resources and requirements for medical research. In 1957, the Resources Analysis Section was created to perform this function within the Office of Research Planning which has rendered staff assistance to the Director on policy-planning matters since its inception in 1949.

Long-range plans estimate the future level of medical research, approximate the resources required, and assess the feasibility of alternative courses of action designed to assure their availability. Because foundations, voluntary health agencies, unorganized private giving, corporations, and the states play such a vital role in the financing of medical research, these plans contain assumptions about the level of future support from nonfederal sources. This does not imply a parity formula for freezing the federal share in perpetuity. On the contrary, long-range plans assume that the federal share should be responsive to the rapidly changing needs of society and shifting scientific opportunities.

Long-range plans often connote fixed goals and a static situation between the present and the distant future. However, by a continuing appraisal of the situation as it exists rather than as it might have been, plans can be revised in the light of changing conditions. Such an appraisal necessarily calls for a look ahead at the problems that NIH must meet in planning its programs for the future.

A LOOK AHEAD

Most of the problems of the next decade are already of immediate concern. These problems relate to the rate of growth and level of medical research expenditures, the challenges beyond the chronic diseases, the shifting policy focus from the individual investigator to the institution as a whole, the strengthening of the nation's research structure, increased emphasis upon co-operative research on an international scale, the financing of medical education and the quality of its product, and the adaptation of organization and management to cope with these problems.

The level of research expenditure

Many unpredictable factors will influence the future growth of medical research. However, assuming continuation of past trends, health research expenditures may again increase four-fold or more within a decade, reaching $2.5 to $3 billion by 1970. Achievement of such levels will demand substantial enlargement of resources, both manpower and facilities. Federal support seems essential to assure the availability of these resources needed to fulfill the national interest.

Beyond the chronic diseases

While the leading causes of death, heart disease and cancer, will continue to receive top priority, we can look for a steady broadening of research in many directions. A society geared to maximizing its potential in the continuing struggle for competitive coexistence will demand research in the etiology and prevention of accidents, the leading cause of death during the first years of life. More research is also needed on those diseases that diminish individual and social productivity, for example, virus respiratory diseases. An increasingly urbanized, industrialized society will support more research on environmental health hazards. Fundamental research will receive increased support to gain greater knowledge and understanding of the basic biological, physical, and social structure of man.

The institution as a whole

The support of individual investigators on a project-by-project basis gradually accumulates into a research "program" for an academic institution, hospital, or research institute. The criterion of scientific merit is adequate for evaluating applications from individual investigators; it will not suffice when one considers the impact of an agency's grants upon an institution as a whole.

To meet this problem, NIH needs new and better ways for dealing with the institution as a whole. Why? Because NIH programs now constitute the primary source of research support at many institutions; because the cumulative impact may strain the institution's teaching capabilities, may weaken its control over its own destiny, may impose unforeseen financial burdens; because NIH desires to foster the more effective integration of research, education, and service; and because NIH aims to strengthen the resources, independence, and integrity of academic institutions, recognizing that a nation's research potential is relative to the quality of its educational system.

Consequently, NIH is actively exploring ways and means for dealing with the institution as a whole. One way would be to secure institutional participation in the policy and program planning process by expanding the advisory structure which now provides representation for fields of science through the study sections and for national attitudes through the Councils. A complementary approach would use institutional research grants to furnish fluid funds and thereby better enable the institution to guide its own destiny.

Strengthening the nation's research structure

The NIH programs for support of research, training, and research facilities have greatly strengthened the nation's research structure. Looking ahead, one can see clearly the need to create regional biomathematical and electronics applications centers, environmental health research centers, germ-free life laboratories and similar, costly, specialized research resources becoming increasingly strategic for advanced biological investigation. A recent report of a good ad hoc advisory group to the Secretary of Health, Education, and Welfare recommended regional dispersal of such facilities so that they might serve many institutions and areas of inquiry.[3] In this connection, the Congress has directed NIH to report upon the need for such facilities, including a thorough exploration of financial requirements.

The weaker institutions constitute another less dramatic but vital deficiency in the nation's research structure. The competition of excellence yields scant benefit to those who require substantial nourishment before their potential can be realized. Indeed, as in all walks of life, those with an advantage tend to increase it; faculty, funds, students, facilities all flow toward institutions with research reputations. More imaginative mechanisms and new criteria must be conceived to build up the marginal producers in our national research and intellectual structure.

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[3] *The Advancement of Medical Research and Education,* Final Report of the Secretary's Consultants on Medical Research and Education (June 27, 1958).

International research programs

In his 1958 State of the Union Message, President Eisenhower called attention to the need for expanding medical research on an international scale. Legislation introduced by Senator Hill to establish a new Institute for International Health and Medical Research within the NIH framework passed the Senate on May 20, 1959, and is now being considered by the House. Following the recent visit of Premier Khrushchev, the Ministry of Health of the Soviet Union and the United States Public Health Service signed an agreement expanding the medical exchange program between the two nations. These developments augur increased emphasis upon co-operative research on an international scale; when realized they will greatly expand both the resources and scope of medical research. Recognizing the shape of things to come, NIH is exploring the needs, the nature of the arrangements which will be required, and the diverse and complex problems which can be anticipated in the administration of a truly world-wide program.

What Federal Funds Mean to the Universities Today

By John C. Weaver

Abstract: The accomplishments of higher education, regardless of their local significance, have been justifiably swept into the public consciousness as matters of grave national concern. As a result, federal aid to our universities and colleges has arrived, and clearly arrived to stay. Neither theoretically nor practically is there anything basically wrong with the concept of such federal aid; indeed, it has become prerequisite to the future growth and attainments of higher education. As things are presently constituted, federal aid is in a position to influence directly the conduct and development of higher education along two basic lines. One of these is through the powerful range of support flowing into our institutions under the multifarious arrangements of sponsored research; the other is through fellowship and loan aid to students, and other provisions of the National Defense Education Act. There can be little argument that our universities and colleges need this help, but it is bringing with it a whole new way of academic life, and there are a variety of influences calling for thoughtful caution and wary appraisal as the transforming tide rolls over our scholarly community.

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HOW did your legislature treat you this year?" Wherever men from state-supported universities gather, even as complete strangers, they need never resort to the weather as a subject for an opening conversational gambit. This is the question that will instantaneously provide a point of mutually absorbing and sympathetic contact. Indeed, among those who toil in the vineyards of public higher education, and rather particularly among those involved in the responsibilities of institutional administration, this "clutch" question and its associated implications reveal preoccupying apprehensions and virtually obsessive concerns.

An adequate understanding of this situation and the trend of its development calls for more than the usual analyses of such contributory factors to spiraling institutional operating costs as national inflationary pressures, mounting undergraduate and graduate enrollments, mushrooming research commitments, and necessary physical plant expansions. It demands more than an appreciation of the shortages of well-trained faculty and the frantic scramble for men that are causing universities to plead with their constituencies for higher and higher salaries in order that they can compete effectively with their sister universities around the nation for top-quality professorial services. And what heavily belabored state legislature has not in recent months heard the agonized assertion: "We must have a substantial increase in salary support if our university is to be able to recruit and hold a competent staff."

Perhaps most fundamental of all in the harassment and brow-furrowing consternation of legislator, benefactor, and educator alike is the direction of taxation that is inexorably increasing the concentration of both private and public resources in the federal treasury. Embattled state legislators, closer and therefore understandably more sensitive to the taxpayer's woes, find their budgets creaking at every joint as competitive pressures for the locally available tax dollar grow in severity. At the same time, the great personal fortunes which initially endowed many of the nation's private institutions of higher learning can no longer form in the face of the powerful prescriptions of the federal income tax laws. Notwithstanding this confiscatory influence, the support of private institutions is being augmented, and increasingly so, by corporate and personal giving in escape of taxes. The need, however, far exceeds the available supply of such donations, in part because of such competition for funds as the establishment of foundations supporting many enterprises other than higher education.

GROWTH OF FEDERAL SUPPORT

If one were to pursue a gross oversimplification of a vastly complex problem, the accomplishments of higher edution, regardless of their local signficance, have justifiably elicited grave national concern among the people. There is ample precedent for pulling national needs under the sprawling federal fiscal tent on political, if not specific constitutional, grounds. Just as the trunk highway system is an economic burden that can no longer be dealt with at the exclusively state and local level, and just as the excellence of our major highways is recognized as serving far more than local demands, so likewise do the costs of achieving a healthy and necessarily massive growth of the facilities of higher education transcend both the available resources and interests of any single state or local community.

Such, then, are the factors that have been responsible for an enormous growth of federal support to the nation's public and private colleges and universities. In 1940, the federal government provided $15,000,000 to the colleges and universities for research and development. Almost exclusively, this was traditional support to the agricultural experiment stations of the land-grant institutions. Less than two decades later, in the fiscal year ending June 30, 1958, federal agencies spent $440,000,000 for research and development and training in a wide variety of public and private colleges and universities. Every year the total continues to rise. The National Defense Education Act of 1958 is adding many additional millions to the federal subsidy of higher education.

Federal aid to education is here, and clearly here to stay. Neither theoretically nor practically is there anything wrong with the basic concept of federal aid to higher education; indeed, it has become prerequisite to the future growth and attainments of higher education. The Northwest Ordinance and the Land-Grant Act long ago established the precedent, and education developed under their provisions has clearly demonstrated the efficacy of the practice.

There can be little argument that our institutions need this money. Indeed, its availability is adding great strength to our general operations. It is providing the direct support for a major proportion of the research of the faculty. This research is predominantly scholarly endeavor of a type which brings a fresh vitality to the classroom and offers opportunity essential to high staff morale. Federal funds also provide much of the expensive equipment and some of the physical facilities which a university presumably would have no other way of obtaining. Modern graduate education is hereby supported and strengthened in the form of assistantships and fellowships, as well as in the challenging and diverse staff research facilities in which the students can participate.

It is apparent that, as things are presently constituted, federal aid is in a position to influence directly the conduct and future development of higher education in two basic ways: the powerful range of support flowing into our institutions under the multifarious arrangements of sponsored research, and the fellowship and loan aid to students, and other provisions of the National Defense Education Act.

INFLUENCES ON UNIVERSITY ACTIVITIES

The outside funds received by an institution in support of sponsored research bring influences that touch university activities, and particularly graduate education, at a number of significant points. This rapidly growing program at a level anywhere near the present, has only comparatively recently come in the back door to take up lodging in the academic household. At some institutions the magnitude of the involvement is colossal; in others it has not even been adequately measured. Significantly, however, almost everywhere, people find themselves somewhat uneasy in its sprawling presence.

Perhaps the prevailing state of apprehension derives in the first instance from the fact that the creature is so enormous, so subtle and persistent in movement and growth, that it is extremely difficult to get one's intellectual "hands" on it. One thing is sure. It has brought with it a whole new way of academic life, and although it is far too complex to permit the generalization that it is either "good" or bad," it has had a staggering impact and is bringing tremendous change. Deep concern undoubtedly also arises from a feeling of sheer helplessness in the face of inexorable forces that seem to be sweeping our institutions along in a transforming tide, over which no one has any measurable influence, let alone effective control.

Along with teaching and public service, research traditionally has been accepted as one of the three primary obligations of a university. Not only does this commitment grow out of the academic values inherent in research itself, but also from the fact that effective teaching and public service are indispensably grounded in research and creative scholarship. According to recent data from the National Science Foundation, two thirds of all research expenditures in our American institutions of higher learning this past year came from federal grants and contracts. The utter dependence of our universities on such governmental aid is now firmly established.

Despite all of the obvious advantages, however, this vast involvement in government-sponsored research is not a completely unalloyed blessing. There is reason for thoughtful caution and wary appraisal of the effects which it is having on the over-all academic effort. As a recent National Science Foundation study rightly emphasizes, we must remember that while the federal agencies have the research and development money to spend, and the universities are willing to accept it, and while both are seeking to operate in the public interest, each group operates in a different area of that interest, and each holds its own distinctive objectives, which in many instances are anything but identical, or even compatible. It is only common sense to be watchful when nonuniversity agencies, whose goals are of necessity not coincident with academically oriented ones, come into a position to influence such basic university functions as research and graduate education.

Certainly no attempt can be made here to explore all of the tangled and crisscrossing avenues of concern which lead out from this central problem, but a selection of them should be given brief mention. As every college administrator well knows, it costs money to accept money. One vital matter to consider, as sponsored research grants mushroom in number and size, is how far is it feasible to let such developments go before the point is reached where further expansion is more expensive than institutional resources can afford? Accurate and trustworthy measuring sticks are difficult to find. In spite of all of the negotiations that have been carried on over recent years, a majority of universities still seem to feel that the indirect costs of government-sponsored research are not adequately covered by the variously computed overhead allowances which the institutions are permitted to charge.

Most universities are quick to concede that while sponsored research is putting a new and substantial drain on general operating funds, it has brought benefits which more than justify the over-all costs. Nonetheless, the size of the venture continues to increase, and inevitably a critical point must somewhere be attained at which any given institution will be compelled to concede that it has reached a degree of saturation beyond which it is unwise for it to go. The problem of weighing advantages against disadvantages and resources against costs is extremely difficult, simply because most institutions have not developed valid criteria. And added to this circumstance are the seemingly omnipresent motives of expansion and empire-building, which all too often lure various academic specialty enterprises to look with favor upon even the most rigid grants and contracts.

Disproportionate emphasis

Another point of general concern with sponsored research is the manner in which it tends to throw some segments of the academic enterprise out of balance with others. An educational institution is under obligation to maintain strength of scholarship in all of the intellectual fields it seeks to cover, the social sciences and the humanities no less than the natural sciences. Because many of their objectives are particular and legitimately different from those of a university, government agencies have placed 95 per cent of the nearly half billion dollars of research money given to universities in the natural sciences. This presents the university with some fundamental problems in providing equality of opportunity and sound morale throughout its operations.

Significantly and fortunately, the men in the natural sciences are as worried about the "have not" areas in the academic scene as anyone. Adequate support of the social sciences and humanities, in the presence of this overwhelming subsidy of science and technology, is a troublesome and difficult matter. To be sure, the thoughtful distribution of research support from the internally derived funds of an institution can do something to alleviate this situation, but it is only a comparatively small number of the financially strongest schools that have sufficient available resources to achieve the proper balance. And all too clearly, unfortunate discrimination is a problem not only in the area of research opportunity, but in the adequacy of the support for teaching facilities and existent academic salary rates as well.

Unhappily, this growing state of imbalance is, at least potentially, even more severe than on the surface it appears to be. If, as is so widely claimed, the government research grants do not carry sufficient overhead to pay the costs incurred by the university in handling them, and the institution is therefore forced to dip into its basic operating funds to make up the difference, those areas which have no direct outside support stand to lose a second time around. It was in recognition of these very problems that the Canadian government made a major appropriation designed to give compensatory aid to the nonscience divisions of their universities. One is strongly tempted to believe that our own federal Congress should give consideration to the same proposition.

It is this area of concern that likewise causes one to wince over the National Defense Education Act provision which largely restricts loan funds to undergraduate students who wish to pursue programs in science or technology. As the 1958 Parliament of Science conducted by the American Association for the Advancement of Science last March so rightly emphasized, "Efforts to advance science at the expense of other fields of learning (will) harm all fields of learning and the society they serve."

Direction of research activities

Aside from the types of financial problems already noted, there are a number of other questions raised by our sponsored-research activities. One issue centers around the effect of these funds on the faculty. A university is anxious to have a staff member pursue those basic lines of scholarly inquiry which are strictly of his own choosing. No academic administrative officer would think of telling a member of his faculty what specific program of research he should undertake. Yet, with money to offer in support of some lines of investigation and not in others, government agencies are in a position to wield a powerful and dangerous influence in "directing" the research activities of many of our scholars. The practice in some institutions of permitting extra-salary compensation from research grants or contracts can place an especially unhealthy monetary lure before the staff. This may well result in specific research projects being selected for economic reasons rather than, as should be the case, specific project work that they are largely unavailable to students, no matter how important and urgent the investigation may seem to be, a real question of the long-run national interest is clearly involved. Many an outstanding teacher is failing to plant the invaluable seed for another generation of scholars, simply because some government agency believed his research services were indispensable to their immediate purposes. We must protect the basic educational resources of our institutions of higher learning from extensive internal decimation of this type. Whenever plans appear which will remove an outstanding professor from the classroom and seminar table, we have an obligation to take a long look at whether the research results will weigh as heavily in the scales of posterity as will the Ph.D. students he will train and who will carry his multiplied skills and insights into the future.

"NEW OR EXPANDED" PROGRAM

Perhaps the problem having the broadest and most significant implications among all of the matters affecting graduate education has to do with the scope of our institutional resources and the wise and efficient use of them. The National Defense Education Act of 1958 specifically and restrictively provides support for "new" graduate programs and the "expansion" of old ones. This action would appear to be based on what is almost certainly a wholly erroneous presumption concerning existing capacities to handle graduate students. There is abundant and persuasive evidence that the top fifty graduate schools of the country could as a group, with a reasonable distribution of enrollment, double the total number of graduate students, at present levels of instructional quality, without major expansions of either staff or physical equipment.

To be sure, the graduate schools of a handful of outstanding institutions have reached or even exceeded the saturation point in some fields, but the universities with broadly established and soundly operated graduate programs in which there are actual shortages of well-qualified advanced students are far larger in number. Unquestionably we need to locate and recruit a much higher percentage of the intellectually capable people who can profit by postbaccalaureate work, but what sense does it make to confine federal fellowship aid to those students for whom a given university can claim to have devised a "new or expanded" program? Surely an overwhelming majority of the people who should be attracted into graduate work would profit most from following already established standard sequences. It is of transcendent importance that we distribute any expanding enrollment on a basis consistent with present, or only moderately extended, institutional capacities to train them.

The compelling need is the stabilization and strengthening of graduate efforts already in existence, rather than the more widespread promotion of new activities. And if federal aid is to be extended to graduate education, why should it be given only to the students? Urgent and defensible as fellowships for prospective graduate students are, the graduate schools themselves merit direct aid, and not just in the oblique manner of erratically distributed and unpredictable cost-of-education payments for students who may be attracted by "new or expanded programs." What validity can there be to the philosophy of increasing graduate enrollment, without simultaneous concern for the institutions that are to receive the students and which wish to raise the basic quality of their offerings?

There is a genuine need for substantial and continuing block grants to graduate schools which they may utilize at their own discretion in the bolstering of staff, the extension of library and equipment facilities, and even the improvement of space. Present legislation being what it is, a university can neither provide a place for students receiving federal fellowships, nor can it become eligible for any aid itself, unless it is fortunate enough to have surplus internal funds with which genuinely to extend its over-all operations, or desperate enough to succumb to the temptation to establish "phonies" or "gimmicks" which can, on paper, pass the scrutiny of the appropriate authorities in Washington.

As the large and financially strong schools grow larger and need more staff, they draw the best individuals they can find away from the smaller and more modestly supported institutions. Not having the financial resources to hold their top people in this frantic competition for talent in a market of short supply, the academic strength of the less wealthy institutions grows steadily weaker and the number of graduate students they can attract progressively dwindles. The long-range result is all too easy to envision.

Surely the last thing the institution of traditionally good quality but modest resources should be encouraged to do in this sort of environment, is further to extend its operations. Many, if indeed not most, schools need to bring all available resources to bear on maintaining quality in the areas they have already chosen to develop. The present federal legislation which calls upon a university to embrace additional operations, clearly invites a thinning out of existing resources and may result in ultimate modiocrity, or worse, throughout the institution.

The already large and rich universities have a vital stake in the well-being of the smaller and less wealthy institutions. Many universities have an important role to play in the total national effort in graduate education, if only they can be given aid to accomplish their mission. We should, therefore, encourage them with material aid to remain, as they have sought in the past to be, strong in specialties of their own selection. Let us then ensure that their thoughtfully developed capabilities are fully utilized. Is this not wiser than to promote the indefinite expansion of a few famous graduate centers, or the costly creation of a new array of competing ones in colleges not now in the business at all? Many a respected graduate school and university is in a fight as urgent as survival itself. We cannot afford to let them lose the struggle.

The Role of the National Science Foundation

By Alan T. Waterman

ABSTRACT: The National Science Foundation was established as an independent federal agency by Act of Congress in 1950. The general purpose of the Foundation is the support of basic research and education in the sciences. Its significance lies chiefly in the recognition by the federal government of the continuing need for the support of science as a peacetime measure, following the striking demonstrations of the applications of science to warfare.

The National Science Foundation is expected "to develop and encourage the pursuit of a national policy in support of the sciences." It holds the view that such policy must be developed with the co-operation of the scientific community, with due regard to governmental interests. Sound bases for policy development are laid by periodic surveys of research and development among colleges and universities, industry, government, and other research organizations. Recommendations combining national policy and the role of the federal government in the support of science are made to the President.

The major programs of the Foundation comprise support of basic research by grant, the award of fellowships, both pre- and postdoctoral, to senior scientists and science faculty, and support of student and teacher training.

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THE role of the National Science Foundation in relation to government and science can best be understood when viewed against the general background of federal support of basic research.

Government support of research and development by contract with outside academic and other nonprofit institutions began during World War II as a result of the government's need for the quick solution of a wide variety of scientific and technical problems. It was intiated by the Office of Scientific Research and Development (OSRD). As the war drew to a close, the President sought from the Director of the OSRD, Vannevar Bush, suggestions as to how the techniques and research experience developed by the OSRD could be used for the national welfare in the days of peace ahead.

In his report to the President, *Science the Endless Frontier,* Dr. Bush strongly advocated the creation of a National Science Foundation, but five years of Congressional debate ensued before the National Science Foundation Act of 1950 was enacted into law.

ORIGINS

In the meantime, the notion of the government's contracting for the performance of research directed to the solution of specific problems expanded to include the support of so-called "basic research," that is research in which the principal motive is the advancement of scientific knowledge. Policies and procedures for the support of original and imaginative research in the colleges and universities were evolved by the Office of Naval Research, established by Congress in 1946, and a support program by means of grants was started by the United States Public Health Service in the field of medical research.

In 1947 the newly established Atomic Energy Commission began a program of research support, and somewhat later the Army and the Air Force initiated similar programs.

These agencies adopted the practice of providing support to research projects at colleges and universities on the basis of proposals submitted by the interested scientists with the endorsement of their institutions. Projects were selected for support on the basis of the scientific merit of the problem proposed, the competence of the investigator, and relevance to the interests of the agency. As a general rule, the agency was aided in the selection process by expert consultants or advisory groups of scientists. Each agency provided support by the means available to it, for example, the military agencies by contract and the Public Health Service—later the National Institutes of Health—by grants. When the National Science Foundation began to operate in 1951, support of basic research by the federal government at nonprofit institutions was probably around $75 million.

The Foundation was created by Congress in 1950 to meet the need for a federal agency to develop national policy in science, serve as a focal point for the support and encouragement of basic research which is carried on largely in the universities and for the training of young scientists. Its principal statutory functions included the following:

> To develop and encourage the pursuit of a national policy for the promotion of basic research and education in the sciences.
>
> To initiate and support basic scientific research and programs to strengthen scientific research potential in the mathematical, physical, medical, biological, engineering, and other sciences.
>
> To appraise the impact of research upon industrial development and upon the general welfare.
>
> To award scholarships and graduate fellowships in the mathematical, physical,

medical, biological, engineering, and other sciences.

To evaluate scientific research programs undertaken by agencies of the federal government, and to correlate the Foundation's scientific research programs with those undertaken by individuals and by public and private research groups.

To foster the interchange of scientific information among scientists in the United States and foreign countries.

In fulfilling these responsibilites, the Foundation has considered its first objective to be the strengthening and encouragement of basic research. After careful consideration, the decision was made to support such research in the same general manner as other agencies, that is, by the support of individual research projects in response to requests. A guiding principle has been to ensure that the Foundation's research programs have the endorsement of the nation's scientists, and to this end liberal use has been made of individual consultants and advisory panels.

Policy Functions

The number and variety of federal research programs prompted the Foundation to consider the responsibilities of the several federal agencies which support extramural research and development in the sciences; its primary recommendations were contained in an Executive Order of the President in 1954. The Foundation, the Order states:

> shall . . . recommend to the President policies for the promotion and support of basic research and education in the sciences, including policies with respect to furnishing guidance toward defining the responsibilities of the Federal Government in the conduct and support of basic scientific research.

The Order further directs that the Foundation shall be increasingly responsible for the support of general-purpose basic research but recognizes, also, the importance and desirability of other agencies conducting and supporting basic research in fields closely associated to their missions. The Foundation is not expected to have responsibility for the applied research and development programs of other agencies, and each agency is accountable for the scope and quality of its developmental effort.

The Foundation's mission with respect to research is primarily the support of a comprehensive program for basic research aimed at encouraging the research most significant to the advance of science in each field. The Foundation also encourages distribution of support—among small as well as large institutions, on a wide geographic basis, and with special attention to promising young investigators. It endeavors, also, to strike an optimum balance among the various fields of science.

Comprehensive surveys

As background data for its own research programs and for policy formulation concerning the role of the federal government in the support of science, the Foundation established a series of studies of the nature and extent of the national effort in research and development. A comprehensive survey was undertaken, for the years 1953–54, of the research and development effort at colleges and universities, industrial laboratories, foundations and other nonprofit institutions, nonprofit research institutes and commercial laboratories, and cooperative organizations. These studies were in addition to the Foundation's analyses of the support of research and development by federal agencies published annually in *Federal Funds for Science*. The whole series carries out the directive in the Executive Order "to make comprehensive studies and recommendations regarding the Nation's research effort and its resources for scientific activities. . . ."

The 1953–54 surveys are now being

repeated in order to measure again the national research and development effort in terms of dollars expended and numbers of persons employed, and to make comparisons with the earlier year. The results of present studies will be useful in discovering trends.

The most striking fact that has emerged from the Foundation studies is the magnitude of the total national research and development effort, which, according to preliminary estimates, is now more than $12 billion annually. Also impressive is the rate of growth, which has more than doubled in half a dozen years.

As would be expected, colleges and universities have the most important role in the conduct of basic research. In terms of dollars, almost one-half of the nation's basic research is carried on in colleges and universities. However, educational and other nonprofit institutions are the source of only 19 per cent of the funds involved. The government supplies the largest percentage of funds, 48 per cent, with industry providing 33 per cent. In terms of scientific and engineering manpower, slightly over one-half of those engaged in basic research are employed at educational and other nonprofit institutions; about one-third are working in industry; and the remainder, about 15 per cent, in the federal government.

A question that concerns us is how the federal government's contribution to basic research can produce fruitful results without encroaching upon the traditional independence of institutions of higher learning. In the development of policies designed to achieve this end, the Foundation is expected to exert a role of leadership with respect to the government's over-all program of basic research support.

Thus we conceive the real significance of the Foundation to be measurable not only in terms of activities supported, important as these may be, but as judged by the course and direction of federal support of basic research and its effect upon progress of science in the country as a whole, both in basic research and in training.

RESEARCH SUPPORT

The Foundation is guided by the twenty-four member National Science Board, and the Director, all of whom are appointed by the President. The research support programs are administered by the Biological and Medical Sciences Division and the Mathematical, Physical and Engineering Sciences Division for the natural sciences, and by the Office of Social Sciences for the social sciences. Of the two methods available to the government for providing basic research support—the grant and the contract—the Foundation elected to use the grant because of the freedom and flexibility it allows the principal investigator and his institution in the utilization of funds awarded. Responsibility for the completion of work for which a grant has been made is shared by the institution and the principal investigator, and freedom in the research of the investigator is emphasized.

It should be noted, however, that the research contracts of the Office of Naval Research and the Atomic Energy Commission have been handled in such a way as to afford much the same leeway and flexibility as the grants administered by the Foundation and the National Institutes of Health.

The usefulness of the grant as a mechanism for the federal support of basic research in the universities prompted the Foundation to seek extension of the grant-making authority to other federal agencies. Furthermore, contracts for basic research entail troublesome administrative problems in the disposition of property and equipment acquired in connection with federally sponsored research in colleges and universities to which the government retained title. As a result of the Foundation's efforts, the Eighty-fifth Congress passed Public Law 934, which extends the grant-making authority to appropriate federal agencies and permits them, also, in the case of basic research grants, to vest title to research equipment with the institution, provided such equipment is not needed for government purposes.

Continuity and stability are most important in the support of basic research. This can be achieved in two principal ways: one, by making the grant or contract for a term of years rather than for a single year and renewable; the other, by setting aside a revolving fund for the renewal of grants or contracts when their term expires, without reference to the annual appropriation. The average Foundation grant extends over a two- to three-year period, but a considerable number of grants are made for the five-year period. Several agencies lack authority to make a grant or contract for more than one year; it is hoped that steps may be taken to provide this authorization, thereby helping to provide needed stability in basic research.

Consistently the Foundation has urged, as a matter of federal policy, that in supporting research conducted in institutions of higher learning, agencies of the federal government, if requested, should reimburse these institutions for full indirect costs associated with the direct cost of research supported.

Loyalty issue

"Loyalty" in relation to the support of unclassified research was another issue that called for fundamental policy decision. The Foundation came face to face with the problem during the period of intense national preoccupation with loyalty and security, when the question was raised of federal support to scientists whose loyalty and security were questioned, even though their research was unclassified. The matter was thoroughly discussed within the context of the grants program and a resolution submitted to the National Science Board.

As approved by the Board in November 1954, the policy is that the Foundation, in its appraisal of proposals for the support of unclassified research, will be guided by the judgment of scientists having a working knowledge of an individual's experience, competence, and integrity. It further states that the Foundation does not knowingly give or continue a grant in support of an avowed Communist or an individual convicted of sabotage or other crimes involved in the nation's security.

Subsequently a Special Committee of the National Academy of Sciences on Loyalty in Relation to Government Support of Unclassified Research considered the whole problem and made its report to the President. In releasing the report, the White House noted that its principles were essentially those which supported the policy of the National Science Foundation. The departments and agencies of the federal government were directed to follow practices consistent with the resolution contained in the report of the Academy's committee.

SUPPORT OF RESEARCH FACILITIES

The growing complexity of modern scientific apparatus has increased commensurately the need for major research facilities. Many of these are too costly for a single institution to procure or maintain without outside support. Examples are wind tunnels, high-energy accelerators and high-speed computing machines. Following World War II, such facilities began to be developed or provided principally by the military services, the National Advisory Committee for Aeronautics, and the Atomic Energy Commission. Gradually, however, the universities accumulated needs for such facilities to be used for basic research. The National Science Foundation made a study and report of federal policy in this area, entitled *Federal Financial Support of Physical Facilities and Major Equipment for the Conduct of Scientific Research.* It then initiated a program to provide universities with capital facilities for basic research purposes. In the case of nuclear reactors and large computing facilities, universities share the costs.

The Foundation has paid particular attention to astronomy, which, as a typical fundamental science, has had little support from agencies concerned primarily with the applications of science. Two national astronomical observatories are being built that will supplement the overworked facilities of private institutions. The Kitt Peak National Observatory in Arizona, being built and operated by the Association of Universities for Research in Astronomy, will provide an eighty-inch reflecting telescope equipped primarily for application of photoelectric techniques to astophysical problems, and a unique solar telescope. At the National Radio Astronomy Observatory, a new intellectual center being established at Green Bank, West Virginia, by Associated Universities, Incorporated, an 85-foot radio telescope presently available and a 140-foot instrument under construction will make it possible for astronomers in the United States to have access to large instruments of high precision.

In the biological sciences, field stations have struggled, in the face of inadequate budgetary support, to maintain their position as research units in the universities and colleges. In 1955, the Foundation, acting upon the advice of special advisory groups, initiated a systematic support program for biological field stations and other specialized biological laboratories. Among such facilities assisted by the Foundation are the White Mountain High Altitude Research Laboratory of the University of California, a Prairie Research Station at the University of Missouri, the Marine Biological Laboratory at Woods Hole, and an international research center, the Naples Biological Station.

What began as a "specialized facility" support program has now been broadened to include the support of the development of graduate-level research laboratories in the natural and engineering sciences. Undertaken on a modest pilot basis in fiscal year 1960, the program is designed to help meet the urgent need of university research laboratories for the physical facilities required by modern scientific techniques and methods. The new program is on a fifty-fifty matching-funds basis.

EDUCATION IN THE SCIENCES

The Foundation has worked to acquire an understanding of the nature and magnitude of the problems that the nation faces in maintaining a supply of competent scientific manpower. The programs that have been developed have been based generally upon the following principles and assumptions:

(1) Quality is of primary importance: The education in the sciences programs has consistently emphasized quality. Selection of fellowship recipients, for example, is based on national competition where ability is the primary factor.

(2) The training of scientists, mathematicians, and engineers is the responsibility of the scientific and educational community: Scholars in all fields of learning have traditionally assumed responsibility for the replenishment of their numbers. The role of the National Science Foundation is one of providing a focal point for federal government concern in science education, and of making available necessary funds to the scientists and science teachers

through their institutions and their associations to accomplish the advances which they agree must be made.

(3) Science education in this era has dual responsibility: The character of our expanding technological society requires that every citizen have enough knowledge and understanding of the substance and the aims of science to enable him to make decisions intelligently. The Foundation's programs stress the importance of imparting adequate knowledge of science to nonscientists, through different kinds of emphasis at various levels in the educational system.

(4) Balance must be maintained between science and other fields of scholarship: Although the Foundation's first responsibility with respect to education in the sciences is to ensure that the necessary level of scientific work can be maintained through the training and support of scientists and engineers, it also feels a moral obligation to guard against overemphasis on science and imbalance in the utilization of our intellectual resources.

The Foundation's programs in education in the sciences are administered by the Division of Scientific Personnel and Education and fall in three principal categories: (1) support of students and advanced scholars in science, mathematics, and engineering, (2) training of science teachers in modern scientific theory and knowledge, (3) development and dissemination of modern and realistic subject-matter materials for instruction.

Fellowship program

A most urgent problem is the support of graduate students and advanced scholars. Those students whose capacity for research and teaching has been demonstrated and who have completed their basic studies should be provided means to continue their work. The predoctoral fellowship program was the first program established by the Foundation and has grown in accordance with evaluations of the relative need. Currently, the Foundation conducts five fellowship programs, exclusive of two additional fellowship programs designed primarily for teachers. These programs provide support for students ranging from first-year graduate students to postdoctoral scholars engaged in advanced study at the highest levels.

Additional significant numbers of graduate students receive support as research assistants through the Foundation's programs of basic research grants.

Direct Foundation support for individual students has been restricted to the graduate level. The evidence indicates that, with scholarships presently available, students in the upper ten per cent of the classes graduating from our secondary schools are generally able to obtain assistance toward financing a college education. The Foundation has not recommended the enactment of science scholarship legislation because it believes that a scholarship program should not be limited to science and engineering.

Teacher training program

Another important part of our program for education in the sciences involves in-service training of science and mathematics teachers at every level. Probably the best known is the support program for summer institutes conducted by participating colleges and universities. During the summer of 1959, 300 such institutes provided subject-matter training for about 15,000 high-school teachers of science and mathematics. Academic-year institutes enable teachers on leave of absence to take a full year of specially designed study. Through the in-service institutes, teachers are able to increase their subject-matter knowledge by study at evening or Saturday classes. A new program of summer fellowships for high school science and mathematics teachers supports selected teachers in independent study in their fields of specialization.

Programs of the Foundation for college and university teachers range from postdoctoral and science-faculty fellowships, which provide support for study and research to summer institutes for college teachers and numerous kinds of short conferences and special field institutes. About 6,000 college and university teachers of science, mathematics, and engineering participated in last summer's programs and in the current academic-year programs.

Curricula and courses in mathematics and science have failed to keep pace with the rapid growth of scientific and technological knowledge during the past half-century. To meet this problem, the Foundation has provided support for comprehensive studies, under the aegis of colleges, universities, and professional societies. These efforts are co-operative, enlisting the aid of high-school teachers to ensure that the new materials are prepared in a way that will meet the needs of students.

Under this program, a fundamentally new approach to the teaching of high-school physics is being developed by the Physical Sciences Study Committee, with present headquarters at the Massachusetts Institute of Technology, in which many of the leading physicists of the country are involved. New materials prepared by this committee are being tried out in selected high schools. Another large-scale effort is being made by the School Mathematics Study Group, with headquarters at Yale University where new courses in mathematics for grades seven through twelve are being developed. A program in biology for elementary, secondary, and college levels is in its beginning stages, and similar efforts are being planned for new course materials in chemistry and other fields.

The Foundation maintains liaison with other agencies of the federal government that have responsibilities in the field of education, particularly the United States Office of Education. The basic methods of approach used by the two agencies are essentially different, and the result has been re-enforcement and strengthening of the total effort.

The Office of Education operates generally through the state departments of education in co-operative enterprises. The National Science Foundation works very largely through the scientific community, dealing for the most part directly with colleges and universities and scientists and science-educators and working primarily with the subject matter of science. To ensure the necessary degree of co-operation, numerous conferences have been held between representatives of the two agencies and by staff members in the operating programs who have common interests. Each agency has an official liaison officer with the other.

CONCLUSION

The foregoing discussion does not cover the Foundation's entire program. Considerable activities in international science and in the dissemination and exchange of scientific information, for example, have not been touched upon. Since it was not possible, in a limited space, to give a comprehensive picture of operations, emphasis has been placed upon activities that have tended to establish policy with respect to the support and encouragement of basic research and education in the sciences by the federal government. Recently, the Foundation published a compilation of some fifty recommended *Science Policies of a Government-wide or National Character.*

Despite the rate at which the federal research and development effort has grown, the government is now better than ever prepared to manage it effectively. Responsibility is assigned in this way: The National Science Foundation with its National Science Board has the primary responsibility for dealing with policy concerning federal support of basic research throughout the country. The Federal Council for Science and Technology, which reports to the President, deliberates on matters of policy-co-ordination and future planning among federal agencies and makes recommendations regarding federal programs. The President's Science Advisory Committee, comprising nongovernment scientists and engineers, considers important scientific and technical matters in relation to government policy, with special reference to national security. The presence in the White House of the Special Assistant to the President for Science and Technology makes available to the President at all times advice and counsel on the wide range of scientific and technical affairs.

The past and present degree of strength and effectiveness of federal programs in research and development should not, however, give rise to complacency. It is clear that the potential of the country in science and technology is far from being realized. Both basic research and the education and training of scientists and engineers require a far greater degree of support throughout the country. In order to realize our full potential there must be widespread public recognition, understanding, and appreciation of the importance of intellectual and scholarly activity, and the pursuit of excellence in all fields of intellectual endeavor. Above all, there must develop a determination to accomplish these ends. Only by these means will the United States be capable of leadership and collaboration among the nations of the world in the vast new vistas that science is disclosing.

The Philosophy and Objectives of The National Defense Education Act

By ARTHUR S. FLEMMING

ABSTRACT: Considered in its historical context, the National Defense Education Act expresses a Congressional attitude that has characterized federal legislation in this field from the beginning of the Republic. The variety and scope of its concerns, the number and size of its grants, and its widespread public acceptance indicate that this Act will exert influences beyond and in addition to its specific defense intentions. The National Defense Education Act recognizes that education is a national unifying force, and it regards an educated citizenry as the country's most precious resource. Its ten Titles are designed to motivate the discovery of intelligent and talented young men and women and stimulate them to devote themselves to the sciences, foreign languages, technology, and in general to those intellectual pursuits that will enrich personal life, strengthen resistance to totalitarianism, and enhance the quality of American leadership on the international scene. The Act is also a contract that reasserts the partnership of federal and state governments in education, the former assisting with funds and counsel, the latter exercising full control over their own educational systems and programs.

Arthur S. Flemming, Washington, D. C., has been Secretary of Health, Education, and Welfare since August 1, 1958. From 1948 to 1958, he served as President of Ohio Wesleyan University. He was a member of the U S. Civil Service Commission from 1939 to 1948. During this period he also served on a number of federal commissions and agencies concerned with government organization and war manpower problems. In 1951 he was appointed Assistant to the Director of the Office of Defense Mobilization and later served as Director. Upon completion of that assignment, he became a member of the Hoover Commission on government reorganization. He is currently a member of the President's Advisory Committee on Government Organization.

THE objectives of the National Defense Education Act have their source in a political philosophy that has consistently characterized federal aid to education from the beginning of our nation's history. This philosophy or, rather, this traditional principle, is based on two empirically derived premises. The first confines federal aid to specific programs devised to help the states solve those problems that bear upon national needs and that surpass the states' capabilities to resolve them unaided. The second asserts the sovereign right of the states to administer educational policies, devise curricula, select textbooks, educate and license teachers, supervise methods of instruction, and in general to maintain, manage, and direct the forms, substance, and tempo of their own systems of public education.

These premises have been constant factors in determining the objectives and scope of every act of Congress in aid of education from the Northwest Ordinance of 1787 to the National Defense Education Act of 1958.

THE GROWTH OF A TRADITION

In our country's formative years, total reliance on local autonomy in the management of schools was a practical necessity. Out of the self-contained and socially isolated nature of American communities in those early years, a unique and powerful sense of local responsibility for the schooling of children developed. Nevertheless, despite this strongly held conviction and the fear of the tyranny believed to be inherent in centralized government, the states accepted numerous grants of federal aid with no consequent loss of autonomy nor diminution of their control.

From the Revolution to the Civil War, the federal government encouraged and financially aided education in the states It endowed higher and common schools with lands and granted funds to supplement these endowments. On no occasion did the federal government seek to control public education in the states.

In 1862, under the Morrill Act, the federal government provided an eventual grant of eleven million acres of land for the endowment of at least one college in each state and territory that would provide much needed instruction in agriculture and the mechanic arts. The recipients of the Morrill Act's benefits, the so-called "land-grant colleges," have not experienced any difficulty in maintaining control of their academic programs. Moreover, the extensive public services rendered by these institutions are proof of the enduring value of federal-state co-operation.

This pattern of federal stimulation without federal domination characterized such subsequent legislation as the Smith-Hughes Act and the George-Barden Act, both of which helped the states to strengthen and extend vocational education. It determined the numerous legislative provisions for education during the depression years, the post-World War II provision of tuitional and maintenance assistance to veterans under the "GI Bills," and programs in aid of federally affected areas. These were all manifestations not only of the national concern for education, but also of the federal government's recognition of the supremacy of state responsibility.

To guarantee the continuing separation of federal and state functions, the national government must constantly be on the alert to avoid jeopardizing the vital role of pluralism and thus of freedom and experimentation in education. Indeed, if it is to remain true to its proper purposes, the federal government must clearly define and delimit the scope of its educational role through unremitting scrutiny of the letter and spirit of proposed legislation lest it be interpreted as permitting trespass on state and local jurisdictional supremacies.

DEMANDS OF THE PRESENT

Present times and immediate circumstances have heightened the national concern for education. In the last half century our society has become an intricate network of diverse but related interests, each dependent on all others for effective functioning.

The obligations of world leadership, the increasing demands of industry and the armed forces for highly trained personnel, the requirements of foreign trade and international diplomacy for personnel having mastery of languages other than their own, and the increasing complexity of our economic and social arrangements have imposed on every school district and on every institution of higher learning insistent demands for a greater quantity and improved quality of educational opportunities and facilities to create an adequate national resource of well trained and intellectually enlightened young men and women.

To help solve these problems and to ease the fiscal burden carried by the states are prime purposes of federal aid to education. Accumulated experience over more than 170 years enables us to perceive that, properly utilized with due regard to state responsibility, federal funds can be useful in many ways to both the states and the nation. The National Defense Education Act of 1958 is an excellent illustration of this fact.

BACKGROUND OF THE LEGISLATION

Marion B. Folsom, then Secretary of Health, Education, and Welfare, appointed a committee to review the recommendations of the President's Committee on Education Beyond the High School and to draw up legislative proposals based upon them. Many of the provisions of the National Defense Education Act had their source in those proposals.

After the bill was drawn, there still was doubt that it could win Congressional approval. The reception and fate of school-construction bills in the years immediately preceding did not provide an optimistic outlook.

Impact of Sputnik

When Sputnik rose into orbit, it aroused more than incredulous amazement; it was a blow to American pride. Yet, its advent had positive consequences. It awakened and spurred us into rigorous self-examination of our total educational system. This in turn provided an excellent climate for the consideration of the provisions of the proposed National Defense Education Act.

NATIONAL DEFENSE EDUCATION ACT

The National Defense Education Act of 1958 was passed by the Senate on August 22 and by the House of Representatives on August 23. It was signed into Public Law 85-864 by President Eisenhower on September 2.

The Act authorizes a billion dollars in federal aid for a dozen separate programs as described in its ten Titles.

The philosophy of the Act is exemplified in the Findings and Declaration of Policy of Section 101, Title I, which states in part:

> The Congress hereby finds and declares that the security of the Nation requires the fullest development of the mental resources and technical skills of its young men and women. . . .
>
> We must increase our efforts to identify and educate more of the talent of our Nation. This requires programs that will give assurance that no student of ability will be denied an opportunity for higher education because of financial need; will correct as rapidly as possible the existing imbalances

in our educational programs which have led to an insufficient proportion of our population educated in science, mathematics, and modern foreign languages and trained in technology.

Title I also emphatically prohibits federal control over the curriculum, program of instruction, administration, or personnel of an educational institution or school system.

Educational opportunities extended

The succeeding nine Titles of the Act are very closely dovetailed. Their relationship is made clear by considering the interlocking contents of the Titles rather than by recounting them in numerical sequence. Moreover, the fundamental significance of the National Defense Education Act as a whole is the more effectively understood if its Titles are reviewed in the context of their unifying purposes. For example, one of the most disturbing educational problems of the day emerges from conditions that force able young men and women to withdraw from high school before their secondary education is completed or to discontinue their education upon graduation from high school. Thus, every year about 200,000 gifted students are deprived of the rewarding disciplines of higher education for which they are eminently well qualified. In consequence, the nation suffers a loss of intellectual competence of incalculable magnitude.

The prevention of such personal and social deprivation is the fundamental objective of the National Defense Education Act. It is, therefore, enlightening to consider the Titles of the Act as having the following progression:

Find through strategically located testing programs especially gifted young people and guide them by means of competent counseling to adopt and to act upon educational plans suited to their interests and aptitudes.

Make it possible for them to enter and stay in college by providing liberal loan programs for students who otherwise would be unable to pay the costs of higher education.

Give them better and more easily accessible educational opportunities, improved quality of instruction in greater quantity, and adequate facilities.

Enrich for them the whole educational process by sensitizing administrative responsiveness to educational needs.

Ways and means

The Titles of the Act are designed to stimulate the achievement of these ends in the following specific and concrete ways:

Title V authorizes and assists in the establishment of testing programs to discover able students, guidance and counseling programs for the purpose of advising them to undertake courses of study best suited to their abilities, aptitudes, and skills, to encourage them to complete their secondary education, and to prepare themselves for admission to colleges and universities of their choice. To ensure the effectiveness of these programs, this Title recognizes the serious shortage of qualified testing and guidance personnel and provides for training institutes to meet demands for competent practitioners in this vital field.

Titles II and IV provide financial assistance to needy students, graduate or undergraduate, and fellowships to graduate students.

Title II authorizes the establishment of student loan funds, administered by the participating institutions, for the purpose of ensuring that worthy students will not be denied opportunity for higher education for lack of money. Loan funds have been allotted on the basis of the institution's particular needs and full-time enrollment. The institutions contribute one dollar for every nine dollars of federal money. Students may borrow a maximum of $5,000 over a five-year period, principal and interest to be repaid over a span of ten years, beginning one year after graduation.

Title IV is designed to foster an increased supply of well-trained college and university teachers by assisting in the expansion of facilities for their training, promoting a wider geographical distribution of such facilities, and granting 5,500 fellowships over a period of five years.

Titles III, VI, VII, and VIII have the objective of creating more and better educational opportunities and strengthening instruction.

Title VI has the specific aim of providing increased curricular opportunities for the study and mastery of the languages of those countries with which the United States has business or diplomatic relations. Title VIII is designed to aid the expansion of area vocational education programs to provide skilled technicians for occupations requiring scientific knowledge.

Title III authorizes financial assistance for strengthening science, mathematics, and modern foreign language instruction. Title VII is designed to stimulate research and experimentation for the sake of achieving a more effective utilization of television, radio, motion pictures, and related media as techniques of instruction.

Title X reinforces and complements the other objectives of the Act by encouraging the development of efficient methods of gathering and disseminating statistical and other data so that state education agencies will be able to provide reliable and current information and thus enhance the efficiency and effectiveness of program planning, administrative policy formation, and the provision of services. This Title also directs the Secretary of Health, Education, and Welfare to develop policies and procedures to achieve the co-ordination which has been needed for so long between the several departments and agencies that conduct federal scholarship, fellowship, and other educational programs.

Although it reinforces the science and mathematics programs assisted by the Act, Title IX is not directly chargeable to the United States Office of Education. It authorizes the National Science Foundation to establish a Science Information Service and a Science Information Council. The latter will advise and consult with the former, and both will have one objective: providing the scientist with the information he needs, quickly and adequately, through indexing, abstracting, and translating materials from all domestic and foreign sources. Programs to develop better and faster methods of disseminating scientific information will also be undertaken.

ADMINISTRATION OF THE ACT

Except for Title IX, the fiscal, technical, and policy aspects of the National Defense Education Act are administered by the United States Office of Education of the Department of Health, Education, and Welfare. About three fourths of the appropriated funds will be distributed as grants to state educational agencies for strengthening instruction in elementary and secondary schools, testing and counseling, providing vocational education, and improving statistical services. The remainder will go to institutions of higher education for student loans, fellowships, institutes to train counselors, and foreign language centers; and to agencies, organizations, and individuals for research in the educational uses of television, radio, motion pictures, and related means of communication.

Before these funds can be made available, however, the Office of Education and the recipient states and institutions must meet certain prerequisites.

To this end, the Office of Education has drawn up regulations for the administration of details not specified in the Act but necessary to assure consistency and impartiality. The Office also has made changes in its organizational structure to meet the demands of new administrative duties.

The states must have authorization from their legislatures to receive and administer federal funds for the purposes of the National Defense Education Act. Moreover, except for the provisions of Title II, the states must match federal funds dollar for dollar. In order to do so, their legislatures must appropriate sufficient funds. In some states, however, local funds may be used for the purchase of equipment and facilities without state legislative action.

Each state is required to submit to the United States Commissioner of Education a four-year plan for using federal funds before it can become eligible as a recipient. No state, however, is irrevocably bound to its first outline; it may make such revisions as changing circumstances impose.

Institutions of higher education that wish to participate in the student loan program authorized by Title II are required to establish loan funds and contribute one tenth of the monies utilized. The participating colleges and universities administer their own programs. Under the provisions of Title IV, colleges and universities are required to establish new, or expand current, graduate programs.

The whole intent of the administrative aspects of the Act is to ensure an equitable distribution of available federal funds, to maintain standards of efficiency, and to assure proper expenditures of federal funds.

THE ACT'S ACCEPTANCE

Although it has been in force only a year, the National Defense Education Act has had enthusiastic acceptance.

Approximately 65 per cent of the nation's 2,000 institutions of higher education, representing 80 per cent of full-time college and university enrollment, are participating in the student loan program, as authorized by Title II of the Act. The federal share, in terms of funds actually appropriated and funds requested for fiscal year 1960, amounts to $61.5 million. In addition to the federal contribution, the participating institutions provide one tenth of this amount, bringing the total to $67.6 million.

The plans of fifty states and territories have been approved under Title III which authorizes the provision of funds on a fifty-fifty matching basis to strengthen instruction in science, mathematics, and modern foreign languages. Federal funds amounting to $51.1 million have been certified for this purpose.

Title IV at present provides a maximum of 1,000 graduate fellowships. All of them were approved in fiscal year 1959. An additional 1,500 such fellowships have been requested and approved for fiscal year 1960.

Plans for the creation of state programs for guidance, counseling, and testing to identify and advise superior students, as authorized by Title V, have been approved for fifty states and territories, and $8.65 million has been certified. This Title also provides for the establishment of guidance and counseling institutes. Fifty colleges and universities were given approval to establish such institutes in the summer of 1959. Funds for fiscal year 1959 made possible 4 academic-year institutes in 1959–60. Funds for fiscal year 1960 will make possible the creation of an additional 67 summer and 26 academic-year institutes.

Under the provisions of Title VI, contracts are being negotiated for the establishment of about 20 language and area centers and 12 summer language institutes and 4 academic-year institutes. Modern foreign language fellowships, also made possible by this Title, have been awarded to 171 graduate students. In addition, 20 research projects were started with fiscal year 1959 funds.

As authorized by Title VII, a total of 68 research grants and 14 contracts for research designed to develop more effective educational media and techniques have currently been approved.

Area vocational programs are being initiated, in response to the provisions of Title VIII, in fifty states and territories for the purpose of training students in technical skills essential to the national defense effort.

Forty-nine states have had their plans accepted under the terms of Title X for the purpose of improving the statistical services of state education agencies.

Of all the aspects of the Act, only the affidavit and loyalty oath requirements have evoked adverse reaction. Included without due consideration of the reactions they might provoke, these provisions, quite properly regarded as inadmissible by the academic community, have proved to be meaningless and discriminatory.

On May 4, 1959, the author addressed a letter to the chairman of the Senate Committee on Labor and Public Welfare urging an amendment of the National Defense Education Act that would repeal those provisions requiring affidavits of loyalty and allegiance.

The following reasons were expressed:

1. The Act's provisions make clear that, without the exaction of formal oaths, persons who belong to or support organizations dedicated to the violent overthrow of the government are not entitled to receive federal funds.

2. The formalities involved in signing affidavits and taking oaths do not deter or reveal disloyal persons. Generally, these persons are not scrupulous about the falsification of legal instruments.

3. The loyalty and allegiance requirements are discriminatory in that they single out educators and students for special treatment and in no sense ensure national security.

4. The argument that disloyal persons who sign the affidavit can be prosecuted for perjury introduces unnecessary legal skirmishing. If a person receiving assistance under this Act is identified as a violator of our internal security laws, he can be and should be prosecuted under pre-existing and current laws that are amply effective in dealing with treasonable and subversive offenders.

A LARGER MEANING

The variety and scope of the National Defense Education Act's provisions make them applicable to many of the most urgent educational needs of the day. Moreover, the Act's wide acceptance by parents, educators, students, and government leaders ensures its nation-wide utilization in solving problems that the states, communities, and private institutions could not attack separately and unassisted. Such adaptability and utilization strengthen the belief that this Act will have positive effects greatly surpassing its immediate defense intentions and thus enrich and reinforce the total educational process.

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POLITICAL THEORY

HERBERT H. HYMAN. *Political Socialization: A Study in the Psychology of Political Behavior.* (A Report of the Bureau of Applied Social Research, Columbia University.) Pp. 175 Glencoe, Ill : Free Press, 1959. $4 00.

Fifty years ago Graham Wallas deplored the harmful separation of the study of politics from the study of psychology and hopefully observed "that there are already signs that it is coming to an end." One would have to conclude from an appraisal of what has happened in the intervening years that Professor Wallas' prediction was rather slow in materializing but that it has now clearly come to pass. Whether the union has been as fruitful as he confidently expected is a matter of conjecture.

Recent reviews of the literature in that amorphous area called political behavior reveal an extraordinary profusion of studies intended to illuminate some aspect of politics through the use of psychological concepts. They show a great deal of effort, a certain amount of ingenuity, and an overall impression of great scatter. As is true with so many fields in the so-called "behavioral sciences," the bits and pieces of theory with which researchers in the area are concerned are so weakly integrated that their studies do not ordinarily relate very strongly to each other. We still seem to be exploring the terra incognita of political behavior rather than plotting the total topography.

In this book Professor Hyman undertakes to set himself at one remove from the search for new data and to seek out "the distinctive and systematic contribution that psychological analysis can make to the understanding of political behavior." He proposes to organize his examination of this contribution around three areas of inquiry, the areas of learning, motivation and emotion, and perceptual processes. The present monograph deals only with the first of these; it is presented as the first of a series which the author has in process.

Dr. Hyman was not confronted with an embarrassment of riches in his attempt to demonstrate the manner in which Americans learn their political attitudes and behavior patterns. Political socialization, as he points out, has been virtually ignored by psychologists. Systematic longitudinal studies of early politicization do not exist. Still, he has unearthed an impressive collection of research fragments which he has organized around his central theme with great skill.

Dr. Hyman does not attempt to apply a very refined set of categories to the evidence which he reviews; the diversity and lack of connectedness in the studies available to him preclude such a possibility. He has, however, made effective use of a rather broad system of abstractions and has taken us a step forward in a research area which badly needs organization.

ANGUS CAMPBELL

Director, Survey Research Center
University of Michigan

SIDNEY HOOK. *Political Power and Personal Freedom: Critical Studies in Democracy, Communism, and Civil Rights.* Pp. xviii, 462. New York: Criterion Books, 1959. $7.50.

Political Power and Personal Freedom is essentially a defense of what Professor Hook calls democratic socialism, which he defines in terms of three characteristics, more profitably reduced to two: first, an economy partially controlled and planned in the interests of political and cultural freedom and, secondly, an economy maintaining full employment, efficiently utilizing all its natural resources, and giving its population a decent standard of living (p. 366). In terms of these characteristics, one would have to say that most countries of the world from the United States to Thailand are, to the extent of their achievement, democratic-socialistic; the differences between them are essentially a matter of degree: to the extent to which their governments have legal control over small- and large-scale industry and services and to the extent to which they have achieved a developed economy.

Professor Hook's defense of democratic socialism—which seems to me his special name for the types of political economies found in the West—involves a sharp and thorough attack on Soviet communism. Soviet communism, he says, is opposed to all the great traditions of freedom in the West, and it can be recognized by three important characteristics. It believes that humanity's welfare depends upon the victory of the proletariat over the world; that the proletariat depend upon the dictatorship of the Communist party to achieve this; and that the Communist party's victory depends upon the dictatorship of its leaders. These three propositions logically involve the conclusion that communism must inevitably attempt to conquer the world by whatever means, fair or foul, and mostly foul, at its disposal. Accept this conclusion and it logically follows, says Professor Hook, that peace or peaceful coexistence is impossible between the Soviets and the Western democracies. This hard opinion undergoes a subtle kind of softening in Chapter 32. Here Professor Hook suggests but does not directly admit that the Soviet countries may modify their dogmas by giving them a new, more libertarian interpretation. Hesitatingly Professor Hook implies that this might mean the practical possibility of peaceful coexistence between the West and the Soviet Union.

Of course, this book is more than a brilliant defense of democratic socialism; it ably covers a wide variety of topics from academic freedom to ritualistic liberalism The most interesting of these discussions are the debates between Hook and Eastman and Hook and Bertrand Russell. The first debate deals with the possibility of democratic socialism; the second with East-West policy and human survival. Despite its liveliness and dialectical pyrotechnics, I think the first is largely a verbal dispute. The second reaches into issues that trouble thinking men everywhere such as how to prevent total nuclear warfare. Although to this "ritualistic liberal," Russell seems to have a more cogent argument, Professor Hook still makes a contribution in terms of the warnings and dangers of a policy like Russell's.

My main difficulty with Professor Hook's book is that, despite his brilliance both of invective and of dialectic, the book never seems to offer a practical policy for action against totalitarianism.

Typographically speaking, the book has a serious fault. For some reason unknown to me, pages 366–398 were omitted, and pages 335–365 reduplicated.

RUBIN GOTESKY

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JEAN MEYNAUD. *Introduction à la Science Politique.* (Cahiers de la Fondation Nationale des Sciences Politiques, No. 100.) Pp. 369. Paris: Librairie Armand Colin, 1959. Fr 1 300.

This book deserves recognition for more than one reason. It is the first full-scale exploration of contemporary political science contributed by French scholarship. It is the work of an able man—academic director of the Ecole Pratique des Hautes Etudes, faculty member of both the Institute of Political Studies in Paris and of the University of Lausanne, and author of *Les Groupes de Pression en France* (1958). What is still more important, the present volume is not only a systematic exposition but also an incisive critique, distinguished by great resources of expression as well as of knowledge. Together with Maurice Duverger's new guide to the research techniques applied to politics, Meynaud's *Introduction à la Science Politique* will make it much harder in France to discount political science as a luxury-import from the United States.

The author begins with a survey of the different types of political analysis, proceeds to the question of a suitable theoretical framework, and then deals specifically with such matters as the object of political science, the focus of study, the sources of data, the range of measurement, and the treatment of variables. He singles out for special attention the place occupied by political science within the social sciences and particularly its relations with economics. A recurrent theme in all of this is the development of a "general theory." The concluding chapter sets forth the conditions which the author deems essential to furnish political analysis a truly scientific foundation.

It is remarkable how well the book succeeds in providing its readers with a cross-section of the methodological literature. For the most part, the footnotes bring together, as should be done more frequently, the best references of different national origin, especially French, British, and American; but the United States column is by far the longest. Although by no means uncritical, Professor Meynaud does not belittle the role that has fallen to American research. On the other hand, he is very sensitive to the possibility that political science may commit itself to the defense of a particular system of government or the values that underlie a particular culture. For both he cites examples in the United States, as he does also for the related tendency to look at the world abroad through glasses different from those used for viewing the scene at home.

Thus it is not surprising that scientific advance and rejection of "moralism" (pp. 330 ff.) are here presented as necessarily linked—at least at the present stage, sternly characterized as the "age of hypothesis" (p. 34). One should add, however, that the author neither denies the formative influence of values nor stipulates for research an unattainable requirement of absolute neutrality toward them. Perhaps we must temper Professor Meynaud's zest for verification with the modest acknowledgment of a condition better than bankruptcy. After all, mankind is in possession, and has been for quite some time, of a considerable body of working knowledge to draw upon in the study as well as in the conduct of political affairs. In the utilization of this knowledge, it is true, great benefit flows from a continuous sharpening of our insights. The discipline of fresh hypothesis and the irreverent testing of commonplace are prime movers in the pursuit of truth. Professor Meynaud's admirable work will add significantly to the momentum which political science needs to become a science of politics.

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BERNARD CRICK. *The American Science of Politics: Its Origins and Conditions.* Pp. xv, 252. Berkeley and Los Angeles: University of California Press, 1959. $5.00.

CHARLES S. HYNEMAN. *The Study of Politics: The Present State of American Political Science.* Pp. xi, 232. Urbana: University of Illinois Press, 1959. $4.50.

To introduce the authors of these two kindred volumes: Dr. Crick, an Englishman and an Assistant Lecturer on Government at the London School of Economics, wrote *The American Science of Politics* under a Rockefeller fellowship at the University of California. Dr. Hyneman, author of *The Study of Politics,* is Professor of Government at Indiana University.

Although Crick's style is heavier and less sprightly and his humor more subtle than Hyneman's, and although Crick uses a largely chronological approach, while Hyneman deals topically with contemporary problems confronting political scientists, they are remarkably in agreement concerning the nature and solutions of current controversial issues within the profession. And each has done an excellent job within the brief compass of two-hundred-odd pages of text.

If there is a single theme central to both these books—and this reviewer believes that there is—it is that of modern attempts, by such groups as the "Chicago school," to develop a methodology, somewhat akin to the "scientific method" employed in the physical sciences, designed to make the social sciences "exact" and primarily quantitative disciplines. In the view of these groups, such immeasurable areas of political science as philosophy, ethics, morality, values and ends have little or no place in the legitimate activities of "scholars," since they cannot be validated by incontravertible evidence

Crick and Hyneman agree, however, that this restricted, descriptive approach is rather sterile and meaningless. For, as Crick points out, it tends to reduce political scientists to mere tabulators of largely irrelevant data and enables them to escape responsibility for value judgments and personal commitments with respect to social ends and means. Admittedly, adds Hyneman, because of the multiplicity of "variables" in human relations, political science may never reach the exactitude or provability of chemistry or physics. Hence, the political scientist must arrive at his "personal preferences" as to values, ends, and means primarily through speculation based upon incomplete empirical data, logical reasoning, and intuition. But, he cogently continues: "People cannot wait until science provides answers to their questions about how to use government to achieve a good life. They will act, and their acts will be shaped by the advice available to them. Thoughtful political scientists can provide better advice than other people on many things relating to government (p 190).

Both authors also consider the relevance of fields impinging upon political science, such as psychology, sociology, economics, anthropology, and geography. and the competence of political scientists to integrate them with what Hyneman defines as the knowledge of "that kind or part of government that speaks through law (pp. 26–27) Both maintain that these cognate areas are indeed relevant, and that political scientists must prepare themselves to deal with them, if maximum understanding of the process of government is to be attained. And although Hyneman expresses apprehension lest political scientists "tackle too much," at the expense of depth and quality, he concurs in Crick's condemnation of overspecialization which the "bureaucratic" American university forces upon ambitious faculty members.

Finally, these authors agree, the contemporary tendency to separate "theory" from the "policy sciences," or, in some cases, to scorn the "unscientific" former in favor of the "scientific" latter, has given rise to much controversy concerning the distinction between "power" and "authority." If the study of politics is to be practically useful, it must answer the vital question: "Power for what purpose?" Crick deplores the type of political science which "loses touch, by scorning history and philosophy, with its roots: the protestantism of conscience against mere power" (p. 227).

Obviously these two men are unfashionably willing to take stands on issues and to urge their colleagues to do likewise. To all of which this reviewer breathes an unscientific but fervent "Amen!"

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ALPHEUS THOMAS MASON and RICHARD H. LEACH. *In Quest of Freedom: American Political Thought and Practice.* Pp. viii, 568. Englewood Cliffs, N. J: Prentice-Hall, 1959. $9.25.

Under the title of *In Quest of Freedom* Professors Mason and Leach have presented a stimulating account of the main currents of American political theory from its background in England of the Puritan Revolution to current controversies over decisions of the Warren Court. The emphasis throughout is on the long debate over the rights of the individual versus the rights of property, but in the course of their exposition, the authors have occasion to discuss, among other things, diverse theories of the eighteenth-century British Empire, the assertion of American independence, the making of the Constitution and its interpretation by Hamilton and Jefferson and their schools, Jacksonian democracy, the controversy over slavery and the nature of the Union, the laissez-faire philosophy of the "Gilded Age," the reforming spirit of the "progressive" era, the Republican reaction of the 1920's, New Dealers and anti-New Dealers, the advocacy of "welfare capitalism" as an alternative to the welfare state, and the as yet unsolved problem of reconciling the freedom of the individual with the security of the state in troubled times like the present. The school desegregation issue is treated only briefly.

In a book whose leading author is a biographer of Justice Louis D. Brandeis and Chief Justice Harlan F. Stone, special interest attaches to the analysis of events of the last fifty years. Possibly the most interesting question which the authors raise and attempt to answer is: Why has a Supreme Court which since 1937 has given legislatures an increasingly free hand in narrowing the rights of property, at the same time swung toward more severe restraints upon the same legislatures when they have attempted to narrow or repress civil rights? An answer, which the authors clearly endorse, is found in a pronouncement of the late Justice Robert H Jackson (quoted on page 556): ". . . the presumption of validity which attaches in general to legislative acts is frankly reversed in the case of interferences with free speech and free assembly, and for a perfectly cogent reason. Ordinarily, legislation whose basis in economic wisdom is uncertain can be redressed by the processes of the ballot box or the pressures of opinion. But when the channels of opinion or of peaceful persuasion are corrupted or clogged, these political correctives can no longer be relied on, and the democratic system is threatened at its most vital point. In that event the Court, by intervening, restores the processes of democratic government; it does not disrupt them "

The volume is for the most part admirably objective, though no one will mistake the authors' position on such questions as nationalism versus state rights or laissez-faire versus public regulation of property interests. The brief Index is less than adequate, and there are a few indications of hasty proofreading.

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H. G. NICHOLAS. *The United Nations as a Political Institution.* Pp. 222. New York: Oxford University Press, 1959. $3.40.

Candidly, the reviewer approached his task with some misgivings, as he has not forgiven the United Nations for what he conceives to have been its responsibility for the dismissal of General MacArthur and for the losses in the recent Korean conflict, the first instance in which American arms suffered actual defeat. Also, some recollection of the proceedings at San Francisco that launched the United Nations led to a search of the Index for any mention of Alger Hiss. Subsequent verbatim reading of the text confirmed the Index. He was not mentioned.

Careful reading, however, led to growing respect for the author, whose organization of highly technical material is excellent and who is doubtless justified in contending that an institution now in its second decade and embracing most nations of the world is not on trial, but should be examined without prejudice for what it is in theory and in practice. Moreover, since the work is institutional rather than historical, only incidental reference is made to achievements or to failures.

Frequent allusions suggest comparisons with the British Parliament. But governmental institutions in a homogeneous country after all have slight resemblance to the Parliament of Man. The United States, with fifty states, comes closer to the political pressures of over fifty nations. Even here the analogy is imperfect. Americans are fellow citizens. Only the most advanced "One Worlders" can ignore their national origin So that the United Nations comes closer to a permanent conference of ambassadors Here the author manifests an excellent comprehension of reality and at the same time great skill in demonstrating the niceties of constitution-building which insure autonomy to and, in some sense, parity with the Five Great Powers and their outnumbering juniors. International jeolousies being what they are, the fashioners of the United Nations have demonstrated remarkable acumen.

The reader will be delighted by frequent evidences of literary skill. This the author possesses in good measure, though opportunities to utilize it are limited.

The United Nations as a Political Institution is essentially a work of erudition. Its value is enhanced by an Appendix which quotes in full the "Charter of the United Nations." There may never be a Gladstone to call that Charter the noblest document ever composed by the mind and pen of man. But Mr. Nicholas leaves no doubt of its significance in man's evolution as a political being.

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STEPHEN S. GOODSPEED. *The Nature and Function of International Organization.* Pp. xi, 676. New York: Oxford University Press, 1958. $7.25.

Dr. Goodspeed has written a useful text which provides a broad survey of contemporary international organizations, their objectives and activities. The approach is primarily descriptive, but there is a substantial amount of analytical material, particularly in summaries, conclusions, or appraisals at the end of chapters. The first part of the book deals with experiments in international organization antecedent to the United Nations, with special attention to the League of Nations and its activities in the field of security. This serves as a general and historical introduction to the United Nations, the principal subject of the volume. The emphasis is upon security matters, with chapters on pacific settlement, collective enforcement measures, and the regulation of armaments. Treatment of the specialized agencies, with the exception of the World Health Organization, is so brief that the student will only be able to get the barest flavor of these international organizations from the material presented. The author may have felt himself limited with regard to the space he could give to them. In any event, the presentation would have been strengthened by devoting less attention to the details of organizational structure and more to the need for international organization in the various functional fields and to the activities of the agencies discussed. The volume also includes an examination of international regional organizations, with considerable attention devoted to the highly significant contemporary organizations in Europe like the Coal and Steel Community and the Common Market. The above matters by no means comprise all the subjects covered in this comprehensive survey of the field of international organization. The focus is broad throughout. There is no attempt to give detailed treatment to any subject.

The most important deficiency of this text is its dearth of conceptual analysis. Collective security aspects of the League of Nations and the United Nations constitute a major part of the book. Yet there is no real effort to present collective security as a concept in international relations and to consider the extent of its embodiment in the basic instruments and in the functioning of these organizations. It is true that concepts in international organization are far from being perfected—this is, of course, the case with regard to the whole field of international relations. Nevertheless, there have been considerable efforts directed toward concept formation in recent years, some of which could be presented profitably to an undergraduate student. Balance of power theory is nearly as essential to an understanding of contemporary international organization as collective security. How can the creation of the League of Nations or the United Nations be explained except with reference to balance of power in theory and practice? It is, of course, basic to an understanding of the North Atlantic Treaty Organization (NATO).

In his analysis of the recent Korean

crisis, the author makes the point that "the United Nations was utilized as a disguise for unilateral action by the United States whereby the policy of that nation was to be made the policy of the United Nations." The use of the collective security apparatus of an international organization as an instrument for the promotion of national interests, narrowly construed, has been a familiar practice of Great Power foreign policy in the past. But Mr. Truman makes it clear in his memoirs that the American response to the North Korean invasion of South Korea was made on the basis of a coincidence of national security and collective security interests. In the present stage of development of the international community, it is probably necessary for a Great Power to lead the international action against aggression. The difficulty, as Hans Kelsen has pointed out, is that there is no objective way of determining whether the Power is leading an international hue and cry, acting, in other words, for and in behalf of the international community as well as itself, or only mobilizing the organization for its own selfish purposes. Intent, therefore, is of prime importance.

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M. S. RAJAN. *United Nations and Domestic Jurisdiction.* Pp. xiii, 679. Bombay: Orient Longmans under the auspices of the Indian Council of World Affairs, 1958. RS 25.

With this pioneer study of a complex and controversial subject, M. S. Rajan, of the Indian Council of World Affairs, has made a substantial contribution to the study of international organization. One may differ, at times, with the author's interpretation and emphasis. Occasionally his language is vague or prolix. But the importance of his subject, his painstaking research and analysis, and the objectivity and thoroughness of his presentation make this volume a valuable addition to the literature of the field.

After a lengthy Introduction dealing with the development of the domestic jurisdiction concept, the author takes up the drafting of Article 2, Section 7 of the United Nations Charter and its legal interpretation. There is much of interest and importance here. A more complete explanation of the content of the domestic jurisdiction article, its vaguenesses and ambiguities, would doubtless require, among other things, a fuller examination of American politics, with particular reference to the views of certain Senators and of the American delegation at San Francisco. The problem of getting the treaty through the Senate was a major preoccupation of the State Department. But with regard to this matter of American intent, we can thank the author for reproducing the following statement of Mr. Dulles at the Conference: ". . . future generations will thank us for what we do in adopting simple phrases and allowing them to evolve as the state of the world, the public opinion of the world, and the factual interdependence of the world, make it necessary and appropriate that it should evolve."

Problems involved in the legal interpretation of the domestic jurisdiction clause are examined in very considerable detail. More attention might have been devoted to the first part of the clause, however. This says that nothing in the Charter "shall authorize the United Nations to intervene" in matters essentially within the jurisdiction of states. The language does not forbid such intervention; it only states that the Charter does not empower the Organization to do so. The question arises, therefore, whether there may be inherent rights, privileges or powers possessed by the states of the international community according to general international law enabling them to intervene under certain circumstances in the domestic affairs of states. It may be noted that the Charter itself expressly recognizes one inherent and collective right: self defense.

The largest section of the book deals with United Nations practice regarding domestic jurisdiction. This the author interprets and evaluates. The evidence is overwhelming that United Nations organs consider themselves able to determine their own competency with regard to the disposition of matters involving domestic juris-

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diction issues. It is also evident that by and large, as Mr. Rajan states, "legal considerations were not of decisive importance to the issue of competence but that, on the other hand, political considerations were." This state of affairs is doubtless due to several factors: first, the lack of agreement on the interpretation of Article 2, Section 7 by the states signatory to the Charter; second, the failure to provide that problems of interpretation were to be resolved by the International Court; third, the fact that many of the most serious threats to international peace and security have arisen out of situations which, it can be argued, are within the domestic jurisdiction of a state; and fourth, the intense power struggles which have rent the international community into fragments during the last fifteen years.

The volume concludes with a note by Simone Dreyfus on French literature in the field of domestic jurisdiction and one on German sources by Gerhard Heuer.

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SOCIOLOGY AND ANTHROPOLOGY

JULIA ABRAHAMSON. *A Neighborhood Finds Itself*. Pp. xiv, 370. New York: Harper & Brothers, 1959. $5.00.

If what happened in the University of Chicago neighborhood has ever happened before, this book still verifies what few realize, that (1) cities need not destroy the characteristic human community; (2) something short of war can bring modern Americans to act together idealistically and effectively; and (3) nothing at the socio-cultural level can be called impossible or inevitable.

In 1949 Hyde Park was a proud middle-aged neighborhood fated to be engulfed by the expanding Negro ghetto that, due to an old pattern of residential segregation, was overcrowded and hence blighted. Those who were blind to the future did nothing; those who saw what was coming moved away and hastened its arrival. Only a very few both saw and denied the future. Julia Abrahamson, a Quaker housewife, came to a meeting with forty neighbors; they learned what was going to happen. Eventually she became the resourceful executive of the Hyde Park-Kenwood Community Conference. This remarkable organization set as its goal a good rather than a white neighborhood and set in motion machinery and ideas that reversed the trend.

This is Mrs. Abrahamson's intimate story of the Conference with enough detail and analysis to provide guides for other communities with race problems compounding all those others of our cities. Any troubled citizen or community leader who wants sharply practical advice should read the book, an exciting adventure story that leads into details of the hard work that lie behind genius and history, and ends with a careful and useful Appraisal.

The history described is the more remarkable because impossible moral issues were frankly faced but left hanging while moral people acted. Those who gain by the saving of the community do not forget but cannot repay others who wished and worked as hard as they only to be squeezed out of it by the "redevelopment" that promises to save the neighborhood. Survivors in a lifeboat from which some jumped to keep it from foundering must live uneasily indeed if the selection of those forced overboard was not even based on principles clear to all. For a moral community requiring consensus, this should have been fatal.

Hyde Park succeeded in doing the improbable not only because an old community of well-to-do, well-educated, resourceful, and determined people wanted to continue to live in it, but because the liberal spirit was strong enough to convince people that their racial prejudices interfered with their interests, and because the action required was consonant with the democratic ideal to which all were publicly committed. It succeeded also because the need to compromise was accepted by enough uncompromising idealists to keep resolution from dying on the sharp horns of a thousand moral dilemmas

It does not happen often; but what this book describes can happen anywhere and series of "issues": theory versus practice in American values and performance, American concepts of administration, the meaning and content of individuality in an increasingly bureaucratized and specialized society, the manner in which the problem of evil is confronted in American society, and evolution of effective values within American society in the past twenty years

The American Style resulted from the conference. It is divided into two parts, the first consisting of the six chapters which constituted the original work papers, and the second a group of formal commentaries, a selection of transcriptions of the informal discussion, and a summary description of "the course of the discussion" by the editor, Elting E. Morison.

It is in the second part that the excitement which the conference clearly generated among the participants shows through. Until this point is reached, the book has a miscellaneous quality stemming from differences in length, form, tone, point of view, and lack of connection among the individual chapters which is disturbing. This is felt in spite of the merit which they separately possess For example, the opening chapter, "American Ethics and Public Policy" by Abraham Kaplan discusses critically the relationship between morality and power and the implementation of morality through political force in an essay of more than 100 pages, not a page of which is wasted. The second chapter, "Theory Versus Practice in American Values and Performance" by J. Robert Oppenheimer, about one-tenth its length, has the *déjà vu* quality of an endowed lecture in a mixed series designed for the thoughtful layman. Other chapters, in the order in which they appear, are "America's Administrative Response to Its World Problems" by George F Kennan, "Have There Been Discernible Shifts in American Values During the Past Generation?" by Clyde Kluckhohn (for this reviewer, the most nourishing single contribution), "Individuality" by Henry A. Murray, and a final chapter, "The National Style" by W. W. Rostow, which establishes with notable success the relationships of the diverse themes and approaches of the preceding essays Professor Rostow's ordering of the earlier discussion transforms the record of what must have been an exhilarating conference into a book.

Contributors of commentary on the chapters are Elting E. Morison, David M Potter, Richard M Bissell, Jr, Richard Hofstadter, and David Riesman I intend a double compliment when I say that the commentary is often as thoughtful and provocative as the original chapter upon which is is based

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W LLOYD WARNER. *The Living and the Dead: A Study of the Symbolic Life of Americans.* (Yankee City Series, Vol 5) Pp. xii, 528. New Haven, Conn.: Yale University Press, 1959. $7.50.

Nearly twenty years separate the publishing of the initial and this final volume of the Yankee City Series. Surely no other community study of this generation has had so much influence on sociologists in particular and on intellectuals in general as has this analysis of a small city on the North Shore of Massachusetts.

Part I of this book deals with the colorful career of Yankee City Mayor "Biggy" Muldoon. His successes and defeats are described in terms of kinship, class, and other symbols.

Part II is a study of the city's 1930 tercentenary celebration. This is perhaps the best part of the book. A great number of ceremonies, historical reconstructions, and other activities comprising the festival are analyzed with reference to the status and myths of the participating groups. In the symbolic representations of the past are indirectly expressed the social organization, aspirations, and anxieties of the present.

Part III turns to the symbolic meanings of Memorial Day, Abraham Lincoln, funeral rites, and the cult of the dead. In Part IV are discussed both sympathetically and analytically the symbols of Protestant and Catholic Christianity—its buildings, ceremonies, and social organization.

The last section on methods of studying social symbolism will appeal mainly to specialists. The other parts, although they seem unduly long and not always clearly organized, should interest not only social scientists but also many laymen.

Such limitations in the Yankee City study stem largely from the one-place, one-time approach characteristic of American community studies. In a modern industrial society, however, many if not most of the decisive forces in a locality are manifestations or local variations of the structure and processes of the national society.

For the theoretical framework of his study of symbolism, Warner relies mainly on the generalizing, nonhistorical approach of such writers as Frazer, Durkheim, Robertson, Smith, Radcliffe-Brown, and Freud. "The influence of the moral order on the creation and maintenance of symbol systems [is emphasized], the influence of the human species as an (organic) animal organization is also stressed . . . . Most religious beliefs are fundamentally based on the simple realities and on the relations of family deities" (pp. 447, 506). In practice, however, Warner devotes much attention to class and economic factors as well as to moral and familial elements.

This book is a fitting conclusion to a monumental community study.

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VANCE PACKARD. *The Status Seekers.* An Exploration of Class Behavior in America and the Hidden Barriers That Affect You, Your Community, Your Future. Pp. viii, 376. New York: David McKay Company, 1959. $4.50.

That the author of *The Hidden Persuaders* should follow with another best seller was to be expected. The same spicy, subtly satirical earthiness carries the reader along on a wave of almost hypnotic persuasion. The author is again sad and frightened at the picture he delineates. His artful innuendos convey a feeling of impending disaster, of national failure. Unless something is done, the country will fall to the "status seekers," who in an era of abundance since 1940, are now allegedly located throughout the United States (p. 255). The lines are formed. Five social classes are definitely emerging. National sclerosis has already begun to set in. To many who have enjoyed *The Hidden Persuaders,* however, this new book may come as a disappointment. So many positive factors are overlooked or ridiculed that one may complete its reading with a bad taste.

Since this is a short review, some questions will merely be mentioned here. For example, competent sociological studies of American classes go back at least as far as the turn of the century. Again, the five classes said to be forming now have already been in existence for a long time. Moreover, the author's use of statistics is sometimes rather cavalier, as in his citations concerning "a vast amount of brain power going to waste" (p. 347). Matters like these lead to the conclusion that as an unbiased analysis *The Status Seekers* is considerably lacking.

To this reviewer, despite such shortcomings, the chief difficulty with *The Status Seekers* lies in its apparent double frame of reference—one realistic and the other fanciful. The first benchmark accepts class distinctions as inevitable and suggests that, within this framework, "much can be done to promote contentment, mutual respect, and life satisfaction" (pp. 329–30).

Two ways to promote this end—by achieving better understanding and by discovering and encouraging real talent regardless of station—are then developed in two concluding chapters. But these are constantly marred by a repeated lapse into the other frame of reference or goal

This goal is to show apparently how far we have deviated from the American Dream—the dream of a classless society. We can endure and prevail, the author concludes, only if we "really believe in our system" (does he mean the classless kind?) and are convinced that it "offers fairer rewards and opportunities for the fulfillment of human aspirations than any other."

The Founding Fathers did state that "all men are created free and equal," a dictum which has received many interpretations. The most realistic, perhaps, is that the early settlers and later arrivals in America were anxious to be free from the oppressive class distinctions that existed in Europe and England as a result of innumerable wars of conquest and enslavements of people; they also wanted an equal chance to develop their talents. But this certainly was not and could not mean a classless society. People of various classes came over on the "Mayflower," and many others have come here since. Classes and groupings have continued to develop in America and within them most people are living wholesome and happy lives. If they seek to better themselves, is that something to be sad about? If in the period of affluence since 1940, they have bought many gadgets, is this to be viewed as frightening, except as the Park Avenue persuaders have coerced them into buying what they do not want or cannot afford?

As Clifton Fadiman sums it up, this new book is most provocative, whether the reader is a status seeker, has "thrown in the sponge," or is a status enjoyer. Most people in this country would seem to be status enjoyers Through their neighborhood groups, their civic and service clubs, their churches, and various other associations and societies, much is being done for human welfare and understanding that could not otherwise be accomplished.

These are matters the author does not emphasize. There are, to be sure, hidden and blatant barriers to honest achievement in America. It is well that they be pointed out. The author does this excellently. His observations are always pithy, and they touch on many obscure matters. *The Status Seekers* is a book everyone should read. A better and more realistic title, however, would have been "Status Barriers: Hidden and Overt," with reference to classlessness omitted. Moreover, in economics classes go hand in hand with division of labor and specialization, and without these a society of abundance would be impossible.

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ANSELM L. STRAUSS. *Mirrors and Masks: The Search for Identity.* Pp. 186. Glencoe, Ill.: Free Press, 1959. $4.00.

This is a stimulating, but discursive essay on identity-formation motivated by the author's dissatisfaction with personality development theories. According to Strauss, psychologists and psychiatrists have erred in underestimating the influence of social structure upon individual behavior, assuming an ingrained and early-formed personality structure, and neglecting the historical dimension in their theories and research. To develop a more fruitful perspective for viewing the traditional problems of social psychology, he combines the symbolic interactional and social organizational approaches. His primary concerns are the social processes from which identity emerges and the symbolic and cultural foundations of its structure. The author does not discuss the organization of identity which he considers of more central concern to psychiatry and psychology. Thus, the essay covers only one facet of identity and does not present a complete theory of personality development.

A basic theme of the essay is the importance of language for human action and identity, particularly "naming" as an act of placement or classification of the self and others. Identities are viewed as being perpetually indeterminate because of constantly changing social contexts. Yet some stability of identity does occur because continuities in the social structure provide, although not assure, continuities of personal experience. Incidentally, Strauss believes that if serious identity discontinuity does result it need not be necessarily hazardous, as usually assumed by sociologists, but may be creative. Throughout the book, interaction is viewed as a symbolic and developmental process, operating within the setting of group structure. Since groups have a past, historical, as well as autobiographical materials must be included in a study of personal identity.

Psychologists and psychiatrists should profit from Strauss' emphasis upon social organization and language in identity formation, while sociologists should find useful his attempt to integrate social psychology and social organization. However, the author's style and approach often obscure his significant contributions. Apparently, he believes that a theoretical essay need not be as rigorous and direct as a research report. Terms are seldom defined, including the key concept of identity. At times, his theoretical ideas become so abstruse and divorced from empirical data that the reader not only has difficulty following them but may even question their soundness. Although Strauss defends his essay style, this reviewer feels its looseness and unevenness weaken an otherwise outstanding work.

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GOTTFRIED EISERMANN (Ed.) in collaboration with Paul Honigsheim, Georges Gurvitch, Frederick Lenz, and J. Allan Beegle. *Die Lehre von der Gesselschaft: Ein Lehrbuch der Soziologie.* Pp. xi, 452. Stuttgart: Ferdinand Enke Verlag, 1958. DM 35.50.

Since the end of the last world war German publishers have tried steadily to overcome the lack of up-to-date sociological handbooks, readers, surveys, and teaching materials in general. The one before us wishes to serve a number of purposes: (1) to fill the gap between traditional German sociology and its international development since the coming of Nazism, (2) to inform interested readers—not necessarily members of the profession—of the present trends of thought, (3) to give to the student of sociology the opportunity to acquire "a general view of problems, and state results concerning his discipline and its different principal branches" (p. x).

Hence we find in the first chapter a "History of Sociology," written by Paul Honigsheim and Gottfried Engelmann which, with the help of only brief indications, achieves a rather comprehensive view. The explanatory statements made in regard to Emile Durkheim and his school appear strange, particularly so in view of the fact that out of an abundant literature concerning these basic teachings quite a number of the most important ones have been overlooked. Written by the editor, G. Eisermann, the second chapter entitled "General or 'Pure' Sociology" presents a remarkably well constructed summary of the different schools of sociological thinking, more precise and much clearer in its definitions than many of the brief surveys which have appeared so far. The chapters "Sociology of Religion" and "Sociology of Art, Music and Literature" have been compiled by Paul Honigsheim, whose special knowledge in these fields is well known. From the pen of Georges Gurvitch comes the articles on "Sociology of Law" and "Sociology of Knowledge." Both might or might not be written specifically for this handbook. In any case, they do not seem to concur at all times with ideas expressed by the eminent French sociologist in his more recent publications. They excel, however, in their systematic way of expounding the development and the different tendencies of the two branches—underlining expressly the frequently proposed theorem of the author, that sociology can only develop properly, when able to collaborate intimately with philosophical doctrines in respect to perceptive knowledge.

Chapters on "Wirtschaftssoziologie" by F. Lenz, on "Soziologie der Politik" by G. Eisermann and on "Agrar und Stadtsoziologie" by J. A. Beegle and Paul Honigsheim complete this valuable contribution to the noteworthy enlivement of sociological activities in present-day Germany. The fact that the book contains no Index may reduce its usefulness to students considerably.

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PHILIP M. HAUSER and OTIS DUDLEY DUNCAN (Eds.). *The Study of Population: An Inventory and Appraisal.* Pp. xvi, 864. Chicago: University of Chicago Press, 1959. $15.00.

This monumental appraisal of the status of demography as a science was undertaken at the suggestion and with the financial support of the National Science Foundation as a necessary first step in the Foundation's new program of providing support for population research In view of the formidable scope of the investigation, the directors commissioned by the Foundation decided to divide the labor by assembling a symposium of papers from twenty-seven individual scholars, in addition to the editors' own substantial contribution. The objectives of the project were "to provide a description and evaluation of the data, the fund of knowledge, the theory, and the methods of demography and its various subfields; to examine its relationship to other scientific disciplines, and to indicate important circumstances bearing on the development of demography in various parts of the world. The successful achievement of these specific tasks would, it was hoped, provide a comprehensive picture of demography as a science and a profession as of mid-twentieth century."

The volume is divided into four parts In Part I, Demography as a Science, the editors themselves have attempted to assess the field by summarizing and evaluating the papers obtained from the contributors, although these papers do not constitute the sole basis for their comprehensive appraisal which runs to almost 120 pages. Part II opens with an historical review of demography on a world-wide scale and goes on to describe the development and current status of demography in a number of different countries selected to "provide representation both of areas where demographic research has a relatively long and distinguished history and of those in which promising beginnings in the development of demography and rapid initial progress have been made in recent years " Part III, Elements of Demography, consists of a dozen papers in which the various substantive fields of demography are taken up, preceded by a comparative appraisal of demographic data available for population research. Here one finds analyses of fertility, mortality, migration, population distribution and composition, as well as other topics which form the core of contemporary demographic research and training Finally, Part IV considers the relationship of demography to several other sciences with which it is closely interrelated. These include ecology, geography, physical anthropology, genetics, economics, and sociology.

The quality of the individual contributions varies but it is a pleasure to report that many of the chapters are excellent. This felicitous result was achieved not only by a judicious selection of contributors, many of whom rank as foremost experts in their fields of specialization, but also by the careful, detailed guidance provided by the editors. Unlike so many other symposia which are woefully inadequate in organization and lack any over-all point of view, this volume greatly benefits from the fact that its editors have firm, outspoken views about the nature of demography and state their position clearly and unmistakably. On the other hand, this also accounts for some obvious defects of the undertaking. As the editors themselves frankly avow, their point of view embodies their personal biases and predilections. Although such frankness may be refreshing, it raises serious questions of scientific propriety. For example, is it justifiable in a symposium intended as "an inventory and appraisal" of the entire field to criticize editorially the psychosocial theories of fertility without at the same time inviting a single proponent of this currently influential "school" to state his point of view?

On the whole, however, the merits of this volume greatly outweigh its defects. For once it is possible to accept the claims made by the publisher on the book's cover without cavil. this book provides an encyclopedic summary of the field of demography, and as such it will serve as a standard reference work for years to come.

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ROBERT W. SMUTS. *Women and Work in America*. Pp. vi, 180. New York: Columbia University Press, 1959. $4.50.

Here is a book on a highly significant topic: the increasing gainful employment of women, especially married women, outside of the home in the United States since 1890. The discussion is reliable, concise, and lucid. Four chapters, totaling 155 pages, discuss the changes in the work women do, the women who work, the demands and rewards of women's work, and underlying values and attitudes. The quality of the material is good, but still $4 50 is a lot to pay for the quantity, 155 pages. The price may reflect the publisher's expectation of a small market.

This book is for the intelligent general reader, not the professional student of the family, for there is little here with which the specialist will not already be familiar: the changes in women's occupations since 1890, mainly a relatively sharp decrease of work on farm, in factory, and household service, and a marked increase in clerical, sales, teaching, nursing, and nonhousehold service jobs. Even more striking is the increase in employment of wives and mothers. In 1890 almost all of the white working women were young, single, of immigrant stock or native farmers, working because of economic necessity. Today all types of women are well represented in the labor force; the young and the middle-aged, the poor and the not-poor, the single and the married—all except young mothers of preschool children and women over 65. As to working conditions, women now receive more pay for less work than in 1890, have more job security, and work under pleasanter circumstances. In 1890, employment for women was widely regarded as a threat to femininity, morality, and male supremacy. Today this concern has been greatly diminished as more recognition is accorded woman's right for self-realization.

All of this is quite familiar, but what makes Smuts' account distinctive is his use of literary, biographical, and other historical sources to illuminate and vivify the narrative. For the general reader this is perhaps the best brief account available of the changing occupational role of women in the United States since the beginning of the century.

This reviewer believes it would have sharpened the perspective of the book if the author had, as an extension of his historical viewpoint, included material on non-American working women. Highly instructive for us is the situation in other industrialized societies, such as the Netherlands where religion limits the number of married women workers. Also the book under review focuses on the causes rather than the consequences of changes in women's work, although the author acknowledges, in a brief reference near the close, that the consequences are momentous.

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HERBERT GARFINKEL. *When Negroes March. The March on Washington Movement in the Organizational Politics for FEPC.* Pp. 224. Glencoe, Ill.: Free Press, 1959. $4.00.

In his efforts to improve his lot the Negro of the twentieth century has lent his support to a variety of programs, among them the vocational training approach of Booker T. Washington, the better-living-conditions goals of the National Urban League, the legal defense and court-case emphasis of the National Association for the Advancement of Colored People and, most recently, the technique of Ghandian resistance as given expression by Martin Luther King's Montgomery Improvement Association. Distinctive among these Negro-betterment programs was the dramatic March on Washington movement initiated in the spring of 1941 by A. Philip Randolph, president of the Brotherhood of Sleeping Car Porters.

As related by Professor Garfinkel of Dartmouth College the March on Washington was a last-ditch effort to focus attention on the plight of the urban Negro worker on the eve of America's entry into World War II. In 1941 the long-existent discriminatory practices in hiring, in on-the-job training, and in upgrading were more aggravating than ever to the Negro as he noted his country's eagerness to contrast the American creed of liberty and equality with the suppressions that characterized the fascist nations. Unable by conference-table methods to induce the Roosevelt administration to take effective action against employment bias, Randolph called for a national Negro March on Washington. This bold proposition could not be ignored. Faced with the possibility of an embarrassing large-scale demonstration in the nation's capital, President Roosevelt yielded, issuing on June 25, 1941, an order—Executive Order 8802—prohibiting discrimination in defense industries.

After this victory Randolph attempted to organize his March on Washington supporters into an ongoing organization. But his spirit of dedication and his great personal integrity were not equaled by his administrative ability; moreover, the March on Washington movement faced the competition of the older Negro-betterment groups, some of whose leaders distrusted Randolph's more forthright "direct action" techniques. In a larger sense, however, the March on Washington movement was no failure inasmuch as it paved the way for "a steady increase in minority-group economic opportunities," and it established among Negroes a greater "collective self-reliance."

Skillfully organized, *When Negroes March* is a thoroughly documented, penetrating analysis of a remarkable mass movement which made use of political pressure techniques to achieve an economic goal. Hence this admirable study should be of interest to the social psychologist and the economist as well as to the political scientist and the historian of contemporary America.

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CLEMENT E. VOSE. *Caucasians Only: The Supreme Court, the NAACP, and the Restrictive Covenant Cases*. Pp. xi, 296. Berkeley and Los Angeles: University of California Press, 1959. $6.00.

"The court is a mirror, an excellent mirror, of which historians for some reason have little availed themselves, of the struggle of dominant forces outside the Court." This observation by Felix Frankfurter, which is used as the epigraph to *Caucasians Only*, very aptly describes the subject of this book. Mr. Vose heeds Mr. Frankfurter's advice, and we see, albeit darkly, as through a glass, the sordid struggle of particular interest groups to maintain what they regard as their best interests.

Mr. Vose focuses his attention minutely on particular court cases, both state and federal, in which the legality of residential restrictive covenants was tested. The result is a chronicle of a forty-years war between the segregating white society, represented in the field by white property-owners associations and real estate boards, and, on the other side, an aroused Negro citizenry, led by the National Association for the Advancement of Colored People (NAACP). The legal battle ended with the momentous decisions of 1948 and 1953 when the United States Supreme Court ruled that the Fourteenth Amendment forbade the judicial enforcement of restrictive covenants. The nonlegal contest goes on, however, wherein white citizens seek through essentially illegal means to maintain the racial exclusiveness of their neighborhoods. Author Vose concentrates almost exclusively on the legal fracas, not on the "reconstruction" which is now underway in so many American communities.

One of the most provocative features of this book, though the theme is insufficiently emphasized, is the role of social science, and particularly sociology, in affecting the development of legal precedent and constitutional law. Although Vose discusses the various forces and variables affecting judicial decision, one wishes he had attempted a more systematic analysis of the interplay of these forces. Among the most important of these forces are public opinion and the research and thinking of scientists, particularly social and behavioral scientists. Which way do the currents of influence flow? Grossly oversimplifying the matter, we might ask, is the "order of opinions" scientific opinion to public opinion to judicial opinion? Or is it something else? Mr. Dooley, in his famous observation that Supreme Court decisions follow the latest election returns, seemed sure of the force of public opinion. In contrast to this view, Henry Steele Commager, quoted by Vose, was of the opinion that, by 1937, the sociological method of basing decision on sociological and economic findings had become "all but the official doctrine of the Court." Although there are many permutations according to which the threefold order of opinions may occur, there is a wealth of evidence which suggests that the order, scientific opinion to judicial opinion to public opinion, is becoming more and more common. It is not difficult to find numerous examples in which the courts, in the face of contrary wishes of particular groups, have outlawed various forms of discrimination, to base their decisions on the findings of the social and behavioral sciences.

The author not only demonstrates considerable legal acumen; he also reveals a good measure of sociological insight Like a good teacher he raises, without actually asking, many basic questions about social and legal processes in a democratic society So many of these questions are evoked by this book that the fact that it is the most penetrating analysis of restrictive covenants seems almost incidental

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PAUL H. JACOBSON. *American Marriage and Divorce*. Pp. xviii, 188. New York: Rinehart & Company, 1959. $12.00.

Students of the family have been painfully aware that the United States is the only major nation without a centralized method for the collection and publication of marriage and divorce statistics. Impeded by budgetary and other considerations the National Office of Vital Statistics (NOVS) has done a commendable job in compiling and publishing such data as are available; nevertheless, social scientists working in the family field have been severely handicapped because of informational gaps in statistics dealing with interracial marriage rates, migratory marriage and divorce, remarriage, children affected by divorce, occupational and educational differentials, and so forth. Jacobson has attempted to "fill in" some of the gaps, and by so doing has come up with one of the most valuable books ever written in the field of marriage and divorce.

American Marriage and Divorce is a classic in its field. The book is a mine of information, and in this respect is not likely to be surpassed in the foreseeable future. It must have taken Jacobson the better part of a decade to collect, tabulate, and organize his material, and he can take pride in the fact that the finished product represents a genuine contribution to the family literature.

In publishing its regular marriage and divorce statistics, NOVS must rely on data that are forwarded by the individual states. Since many of the latter have not had centralized reporting procedures, and since, over the years, statistical recording has varied a good deal from state to state, the NOVS "clearing house" has been able to publish only fragmentary and relatively nondetailed information. Jacobson carried out the painstaking task of collecting not only centralized information but also of reaching down to the city, county, and local court level in order to collect and present statistical series not heretofore available.

The net result is a sorely needed compilation of facts dealing with the seasonal pattern of marriage, trends in the marriage and divorce rate, marital patterns by race, age, locale, and occupation, as well as geographic variations and urban-rural differences, marriage and divorce proceedings, alimony, custody of children, mortality and the duration of marriage, and a host of other factors that are of concern to family sociologists. Although not written within any theoretical framework, Jacobson's volume is more than a collection of statistical facts The facts hang together, and the accompanying explanations have been clearly thought out by the author. In consequence, the book will appeal to both the statistical and nonstatistical-minded reader.

The book sells for twelve dollars, and doubtless there will be some who feel the price is too high. Viewed from this corner, the price is right. As a matter of faith, if you are willing to take the word of an old, experienced horse trader, the book is a bargain.

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JEROME K. MYERS and BERTRAM H. ROBERTS. *Family and Class Dynamics in Mental Illness*. Pp. ix, 295. New York: John Wiley & Sons, 1959. $6.95.

This volume is the second part of a research report on the relationship between social class and mental illness. The first part, *Social Class and Mental Illness* by Hollingshead and Redlich, demonstrated that the prevalence of mental illness, its diagnosis, and the kinds of psychiatric therapy were all associated with the patient's position in the class system. Relying on an epidemiological approach, with the use of mass data, no attempt was made in the first study to throw light on the question of how patients become mentally ill, nor on the social and psycho-dynamic factors in the development of mental illness. These are the problems attacked in this study.

Specifically the research seeks to test two hypotheses: (1) social and psycho-dynamic factors in the development of psychiatric disorder are related to the patient's position in the class order; (2) mobility in the class structure is associated with the development of psychiatric disorder. These two propositions were tested through an intensive study of fifty patients, half schizophrenics and half psychoneurotics, carefully selected from two social levels, a lower middle and a lower working class. The research was carried out by a team of social scientists and psychiatrists in a New England metropolitan area, and covered a period of ten years.

The findings of the research completely support the first hypothesis. In part they also support the thesis that a significant factor in the development of illness is the tension associated with mobility in the class structure This was found to be true in the middle but not in the lower class. Statistically, significant differences between middle and lower class patients were found in intrafamilial role relationships, in sex role development, in kind and intensities of community pressures, and in attitudes toward mental illness, as well as to therapy, and in development of symptoms. At each class level, external pressures and internal tensions were more severe and more frequent among schizophrenics than among neurotics. Among middle class patients, pressures clustered around two basic value systems, respectability and success. Out of these developed inner tensions arising out of conflicts between instinctual, especially sexual drives and moral values as represented by parents, especially mothers. In other cases they grew out of the failure to live up to the rigidly internalized values of middle class culture. Tensions associated with the strivings of the upwardly mobile, or fear of loss of status, once achieved, were marked correlates of mental breakdown in the middle class patient.

In the lower class the picture was quite different. Here the pressures of economic adversity and exclusion from participation in the life of the community were the sources of the inner tensions. Lower class, much more than middle class patients, displayed a family history marked by absence of affection and warm interpersonal relations, and by conflict with and fear of parents. Excluded from community participation there was little internalization of conventional norms, hence fewer inhibitions and little guilt feeling over violation of convention. Constant conflict with teachers, social workers, police, employers, and the like, bred feelings of hostility but at the same time of rejection and self-worthlessness. In both cases intrafamilial and community pressures bore more heavily on the schizophrenic than on the neurotic. Both intensity and frequency of pressures were directly related to the severity of mental illness.

The authors repeatedly emphasize that they do not take the position that social class is the only or even the most important factor in mental illness. Psychiatric disorder is confined to no one class. Rather, membership in certain social classes exposes persons more frequently and more intensely to the kinds of pressures which are closely associated with the development of functional mental illness. At the same time most persons exposed to these same pressures do not succumb. But interpretation of the etiology of mental illness and its successful therapy can be achieved only in light of understanding the role of class background.

Family and Class Dynamics in Mental Illness is a worthy companion to the Hollingshead and Redlich volume. It should be read not only in academic circles but by psychiatrists, clinicians, social workers, and laymen who have an interest in the problems of mental health. It has important implications not only for the student of mental illness but for those interested in delinquency and crime as well as for other forms of deviant behavior. The book demonstrates again the importance of expanding the horizon of research in the mental health field to include the contribution of the social scientist as well as those whose orientation is purely psychiatric and clinical.

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DERRICK SHERWIN BAILEY. *Sexual Relation in Christian Thought.* Pp. viii, 312. New York: Harper & Brothers, 1959. $4.50.

Since Comte there has in general been no close meeting of minds between theologians and sociologists. The present book by an Anglican theologian does, however, command respect for its impressive scholarship. Sociologists who have written about historical aspects of the family from secondary sources could well be humbled by Dr. Bailey's command of Hebrew, Greek, and Latin documents interpreted in the light of Biblical criticism.

Chapters of the book cover primitive Christianity, the New Testament, the Patristic Age, the Mediaeval Western Church, the Reformation and seventeenth-century Anglicanism, the tradition today, and, finally, present a theology of sex. There is a lucid analysis of the Christian tradition concerning the relations between men and women as influenced by countless writers, many unknown to the average sociologist. The views of Jerome and Tertullian concerning lust and women are placed in perspective, but Dr. Bailey freely grants that the Christian tradition by virtue of antagonism to a pagan world and for other causes was deeply pervaded by hostility to genital sex expression and by androcentricity.

Much can be learned from this book concerning concepts and disputations over the ages which left significant residues in civil laws and church doctrines of the present day. Affinal relations, impediments, consent, consummation, sacrament, indissolubility of marriage, sacerdotal celibacy, digamy, and "syneisaktism" receive especially illuminating treatment.

It is only toward the end of the book that Dr. Bailey seems other than an objective, scholarly historian familiar both with ancient documents and modern biological and social science. The theologian finally emerges, influenced somewhat by Barth and Buber, lucid, aware of semantic pitfalls, eager and prepared to reason profoundly in a liberal tradition from certain assumptions. The unstated assumptions seem to be a Creator God, a divine Son, special creation of generic Man, and authoritative revelation in Scriptures theologically interpreted. Man is seen as an image of God and as Mankind having equal masculine and feminine components. Man and woman are complementary in a richly variegated association, the potentialities of which are known only by experience free from preconceptions of culture and tradition.

Perhaps sociologists and theologians need not be on widely separate planes of human thought.

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JOHN VINCENT BARRY. *Alexander Maconochie of Norfolk Island: A Study of a Pioneer in Penal Reform.* Pp. xxi, 277. New York: Oxford University Press, 1959. $6.50.

In this book, Mr. Barry attempts to negate the prevailing legend of Maconochie's failure and to elevate him to his rightful place of distinction. Maconochie, of course, was the superintendent of the penal colony on Norfolk Island, over 900 miles east of Sydney, Australia, from 1840–44. Mr. Barry succeeds eminently in his mission.

Many in the United States have assumed that Maconochie had much to do with establishing the "ticket-of-leave" system, which has been considered a precursor of the parole system in America. No less authorities than Barnes and Teeters have labored under this erroneous impression as has the reviewer. From Barry's documentation, it seems clear that ticket-of-leave antedated Maconochie's advent to Van Diemen's Land in 1837 and his superintendency at Norfolk Island. Maconochie did use ticket-of-leave more constructively than heretofore.

The mark system was really the core idea of Maconochie's penal philosophy. Maconochie contended that "prisoners should be punished for the past but trained for the future" The offender would be incarcerated until he had performed a specified quantity of labor. This idea would make imprisonment a task rather than a time operation In addition to the mark system whereby the prisoner earned marks by his labor, Maconochie introduced on Norfolk Island the system of group responsibility: prisoners who had proved themselves ready were assigned to parties of six who were collectively responsible for the performance and misconduct of their members. This was hard to administer. Furthermore, Maconochie introduced several other progressive measures which sought to rehabilitate prisoners and create a good social climate for personal improvement.

From reports, it appears that Maconochie had a most favorable impact on the Norfolk penal colony and that after his removal it fell once again into a subhuman jungle. American penologists will also be interested to note that Maconochie had a rather direct influence on Sir Walter Crofton's development in approximately 1854 of the progressive stages system in the Irish prison system.

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L. S. PALMER. *Man's Journey Through Time: A First Step in Physical and Cultural Anthropochronology.* Pp. xv, 184. New York: Philosophical Library, 1959. No price.

Man and Culture are in a sense synonymous, yet "Man's cultural development has proceeded in a manner quite different from his physical development." Palmer aims to evaluate progress in each area. Three problems in the physical area at once arise: (1) the unit of evolutionary time; this is now solved by accepting a "Darwin" as a unit, that is, "when any dimension changes by 1/1,000 of its original value in 1,000 years"; anatomical indices were not developed with reference to measuring evolutionary change; no particular skeleton is available as a standard. Palmer adopts an "average modern European adult male." In the cultural area there is an obvious limitation to objects, largely stone, rather than concepts and values. Man, the toolmaking animal, is Palmer's over-all concern.

After developing mathematical formulas to equate physical and cultural rates with time, the author establishes four major criteria in each area. In the physical they are: I1, Nuchal Area Height Index; I2, Condylar Position Index; I3, Cranial Capacity; I4, Lower Dental Arcade Convergence Angle. I1 and I2 are related to erect posture, I3 to brain, I4 to speech. In the cultural they are: C1, The Number of Different Materials Used; C2, The Number of Different Occupations Engaged in; C3, The Speed With Which Man Moved by Mechanical Means; C4, Cultural "Grading of Man-Made Objects." C1, C2, and C3 relate to concrete objects and methods and are largely quantitative; C4 is qualitative.

The major inferences on the physical side suggest that erect posture, brain size, and speech developed in order. All graphs extrapolated to several million years ago come close to the values given for Proconsul. The rate of evolution is about one "Darwin," and for periods not exceeding 500,000 years the approximate equation $H = (100\text{-}\alpha t)$ expresses the relation between "percentage humanity" and time (Table II and Fig. 53).

On the cultural side it is concluded, on the basis of graphing the data, that the similarity of the graphs demonstrates that the four criteria chosen measure "the same cultural factor." Further, the shape of the graphs betokens an accelerating developmental process that is currently in a terrific phase of acceleration (Tables III-IV and Fig. 54).

Dr. Palmer has brought together a lot of data and has developed a very useful evaluatory and discriminatory technique. I think we can all agree with his major objective of relating rate of development, physical or cultural, to time. We can argue an almost unsolvable problem, that is, the weighting of the criteria used. On the physical side are nuchal area, occipital condyles, cranial capacity, dental arch the best single measurements to evaluate fundamental evolutionary biological change? If so, which tells the most? If not, what others should be considered? The same questions may be asked of the cultural criteria which range from flint-tipped spear and bone needle in carved bone case to supersonic jet plane and radar-controlled automatically fired antiaircraft gun battery. Palmer's method equates and rates all of the criteria chosen in each area, to be sure, but the choosing of the traits is the moot point. Incidentally, by implication at least, cultural evolution is assumed.

The book is clearly written, well illustrated, and excellently printed. It should be read, studied, and evaluated by all biologists and all anthropologists

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EDWARD NORBECK. *Pineapple Town: Hawaii.* Pp. xii, 159. Berkeley and Los Angeles: University of California Press, 1959. $4.00.

In this book an anthropologist examines Maunaloa, a pineapple plantation community on the island of Molokai

After a sketch of the growth of Hawaii's comparatively young pineapple industry, built upon imported labor, mostly Asian, the author describes the scientific and highly mechanized operation of the Maunaloa "factory farm" and reports on the ethnic backgrounds, skills, and rewards of the corporation's employees. There follows a generalized portrait of the town, with attention to such factors as the racial and cultural composition of the residents, education, religion, languages used, town layout, housing, and community facilities.

Because the peoples of the community still "remain culturally as well as socially distinct," the Filipinos, Japanese, Haoles (Caucasians) and Cosmopolitans—persons of mixed national and cultural backgrounds —are next examined separately, with attention both to "old-country" attitudes and customs which have persisted within the first two groups and to forces which are gradually obliterating various of these survivals Of prime importance in the latter connection is the corporation's pressure toward elimination of attitudes and habits inimical to efficient industry.

Though they are to some extent influenced by the racial and cultural affiliations of the individual, the author maintains that social classifications are very largely determined by occupational status Though Haoles still have both top jobs and social prestige and are followed in rank by Japanese and then the Filipino majority, it is stressed that this ranking is based not solely on "race" but on cultural considerations which derive partly from corporate needs and partly from social traditions uniquely Hawaiian. Opportunities for, and obstacles to, individual upward mobility are described in some detail. The non-Haoles chances for advancement have greatly increased in recent years

The final chapter summarizes changes in community social relationships since the plantation's origin in the 1920's. These include trends away from paternalism and social solidarity by kinship and ethnic grouping toward impersonality in economic relations, individualism, and a class-structured society. They are viewed not solely as the results of a general diffusion of American culture, but as the unplanned "functional correlates of industrialization," which can be observed in other Hawaiian pineapple towns, and reflect similar tendencies in plantation owned communities in other areas of the world

Readable and well organized, this book provides indubitably authentic, and sometimes most interesting detail on the lives and relationships of Maunaloa's residents. The chapters on "The Filipinos" and on "Community Social Relationships" are especially noteworthy. Photographs, maps, statistical tables, and an Index are provided.

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ARTHUR R. M LOWER. *Canadians in the Making: A Social History of Canada.* Pp. xxiv, 475. New York: Longmans, Green & Company, 1959. $7.50.

Perhaps every historian after a certain age becomes a philosopher. He reaches a time in life when he wants to reflect on history and leave to younger men the work of filling in the small, scattered gaps in historical knowledge This, certainly, would appear to be true of the distinguished Canadian historian, A R. M. Lower. The author of *Colony to Nation* might well have been expected to turn to a work of an even more general nature, a history of the Canadian people, and no historian had more earned the right Only one with the breadth of historical knowledge and the insight of Professor Lower could have written *Canadians in the Making* This is a good book for the reason that nothing that Professor Lower writes can be anything but good.

Had a lesser historian, however, undertaken to write such a book the result might well have been disastrous. It would in no way please Professor Lower who reserves some of his most acid comments for references to sociology to suggest that to write the history of the Canadian people he needed to be more of a sociologist than he was. Professor Lower fancies himself as something of a philosopher of history, and *Canadians in the Making* opens on a note that suggests that there, in a sort of stage-theory of development, has been discovered the key which will give meaning to the history of the Canadian people. But as attention shifts from the early years of the growth of the Canadian nation to the more complex developments of the late nineteenth and early twentieth centuries, the stage-theory is fortunately forgotten and what might be described as a philosophical approach disappears. What develops in its stead is a sort of rambling commentary on Canadian life, the social class relations of the Canadian people, their style of architecture, their religious beliefs and ethical practices, their cultural achievements, and such. There is much good history crowded into even the later sections of the book, particularly history with a strong local flavor, but as the story of the Canadian people unfolds, and the guideposts directing historical analysis become more obscure, the temptation of the author to give free rein to the expression of his views about various things becomes irresistible. By the time Professor Lower has done, there remain few facets of Canadian life on which he has not passed judgment.

Those familiar with Professor Lower's earlier writings, and particularly his scattered articles and papers, will discover much in *Canadians in the Making* that they had read before. But there is a pungency about whatever he says that can scarcely fail to make what he writes interesting. Few sacred cows are left peacefully munching their hay after Lower has done with them. Never one to criticize his neighbor without subjecting himself to the same kind of searching analysis, Professor Lower directs his most deadly barbs at that Canadian society of which he has been most a part. However, he says little that is unkind about his French-speaking Catholic compatriots. No one perhaps who was not intensely Canadian could have written *Canadians in the Making*. And it is only those few perhaps who are as deeply attached to their country as is Arthur Lower who will fully appreciate this book

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DAVID L. MILLER. *Modern Science and Human Freedom*. Pp. xiii, 297. Austin. University of Texas Press, 1959. $6.00.

This volume, seeking to resolve the oppositions between the deterministic postulates of science and man's freedom to set goals for himself and devise means to achieve his objectives, is in fact a complete philosophy of science. To prove his theses, Dr. Miller finds it necessary to investigate, in Part I, The Physical Bases of Science, and here problems of the place of Observables, Explanation, Laws, Cause, Valuation, and related scientific debatables, are dealt with—in the course of which Positivism fares badly. The discussion is clear and comprehensive.

In Part II, The Ethical Bases of Freedom are discussed. Problems concerning the nature of life, biological organization, evolution, the place of mind, and the like, are surveyed. The discussion, in Chapter XII, of problems in the social sciences is related to the concepts of freedom, institutions, and values, which then leads into the investigation of a possible science of norms, and here the previously hard-won conclusions are brought to a focus. In the formulation of his philosophy Dr. Miller was, and is, much influenced by the views of his former teacher, George H. Mead Following this approach, Dr. Miller indicates, in terms of physiology and psychology, how human freedom emerges in a natural way as the culmination of evolutionary processes wherein the symbolic process and reflective thinking make possible planned behavior of individuals as members of society. The co-ordination of the symbolic function with bodily behavior—as man acts on physical objects and social processes to create and transform ends (goals) —is the source of man's freedom.

This book appeals to me as an excellent survey and affirmation of a viewpoint. My major criticism of it relates to the limited manner in which the author states his problem: one of his aims (pp. 3, 4, 12, and *passim*) is to reconcile the Greek rational tradition, as the historic source of science, with the basic tenets of the Hebraic-Christian tradition which, the author believes, gives us our cultural components of freedom, responsibility, and the hope of improving the world through human effort and ethics. Surely this frame of reference is too narrow and limited for modern science and philosophy. Is not our task precisely that of rising above this traditional and obsolete antithesis to a higher plateau of thinking whereon we may discuss and settle problems on the level of a new world synthesis? Our coming civilization, if it comes at all, will not be merely "Western" —it will represent a planetary level of integration in which the Greek-Hebrew-Christian provincialisms will be transcended. Given his own context, the author has produced a useful and readable volume.

OLIVER L. REISER

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ECONOMICS

MASSIMO SALVADORI. *The Economics of Freedom: American Capitalism Today.* Pp. xxii, 242. Garden City, N. Y.: Doubleday & Company, 1959. $4.50.

Professor Salvadori's book is "written by a layman for laymen . . . on the assumption that . . . any citizen can acquire the knowledge necessary to an understanding of an economic system." This word "system" is central to the book's whole scheme of exposition. Our "people's capitalism" is a philosophical system, not circumscribed by naturalistic scientism, as were the doctrines of the classical economists, deductively constructing their dismal science. Ours is a positive system based on the constructive use of individual liberty. He says, "Some Americans and a good many foreigners, baffled by the complexity, the apparent contradictions, and the variety of the American economy, maintain that there is not a system but simply something which has grown without direction and without purpose, which holds precariously together and which is likely to disintegrate in the near future. . . . Clearly they are wrong: we know intuitively that the system exists and we know empirically how it operates—there must therefore be something holding together the elements of which it is composed" (p. 31).

This system in its material aspect (Chapter 4) embraces the institutions of property, capital, free enterprise, and the market. In terms of "man as an economic agent" it embraces (Chapter 5) labor organization, business administration, and government guidance. The "driving forces" (Chapter 6) behind or within the system "that represent the core (one may say the soul) of our economic institutions" are individualism, rationalism, liberty, equality, respect for work, materialism—in its best sense, solidarity, and evolutionary revolutionism.

Out of all this unsystematic system we have, in Salvadori's estimation, achieved something that "as it is—with its innumerable defects, weaknesses, and dark spots, not as intellectuals, businessmen, labor economists, [and so forth] would like it to be—represents an improvement over all other economic systems past and present. . . . If Americans make a mess of their lives, as many do, it is of their own doing, not the responsibility of the economic system."

This book is an essay in persuasion and, as such, is a forceful and inspirting document. But to this reviewer it would seem that if the general reader is to understand the "system" of people's capitalism he must have more in the way of concrete exposition of its modus operandi—for instance, its credit structure, the nature of professional management of corporations and professional administration of unions and their federations, and the income redistributions characterized by individual and corporation taxes and social security payments and business subsidies.

EDWIN G. NOURSE
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JOHN M. LETICHE. *Balance of Payments and Economic Growth.* Pp. xiii, 378. New York: Harper & Brothers, 1959. $6.00.

One of the more interesting recent explanations of the dollar shortage links the strong payments position of the United States to greater rates of productivity growth in American import-competing industries than in foreign exporting industries. That this can serve as a partial explanation of Britain's postwar dollar shortage but definitely not as a general explanation of the difficulties faced in Western Europe is one of the fundamental points Professor Letiche makes in his new book. Recent hypotheses are examined by him with the aid of interwar and postwar data for the United States and Europe.

The relationship stressed by Professor Letiche is that between actual levels of output and full capacity levels of output. In his opinion, the United States is distinguished from other countries in its ability to generate excess capacity. This tendency, according to the argument, results in an excess of domestic production over domestic expenditure, which, in turn, is the export surplus. To the extent that the export surplus is not matched by an outflow of capital, that is, foreign investment, the rest of the world suffers from a dollar shortage. The weak link in this chain of reasoning, insufficiently supported in this book, is the assumption that excess capacity is associated with an excess of actual production over domestic demand.

Only the second part of the book is devoted to exploring the relationship between economic growth and the balance of payments. Part One is a critical appraisal of classical, Keynesian, and neo-Keynesian analyses of the process of adjustment to disturbances in the balance of payments. He rightly points out that adjustments usually involve many prices, employment, and incomes, but seems to go too far in rejecting traditional arguments couched in terms of elasticities of demand and supply for exports, imports, and foreign exchange. In working out a "more general mechanism theory" Professor Letiche ignores recent work by economists wedding the elasticity arguments with income effects and essentially repeats much of the elasticity argument in different terminology.

The book attempts to reach the nontechnical reader, by minimizing the use of algebra and symbols. However, in so doing Professor Letiche runs the risk of not satisfying the technicians without communicating with the others; his long verbal accounts of the adjustment mechanism (for example, pp. 131–149) will be difficult for the uninitiated. The discussion of Part Two, however, should be of general interest to all concerned with the development of the present payments position in Britain, Western Europe, and the United States.

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RENDIGS FELS. *American Business Cycles 1865–1897.* Pp. 244. Chapel Hill: University of North Carolina Press, 1959. $6.00.

In this useful study of business cycles in the United States from the end of the Civil War to the end of the century, Rendigs Fels helps fill a major gap. Save for unpublished dissertations and articles in journals, there has been no book dealing competently with the cycles from 1865 through 1897. Drawing on the work of the National Bureau of Economic Research, on contemporary accounts, as well as on various though not all available sources of information and analysis, Fels has managed to join in felicitous fashion empirical data and theoretical analysis which has illuminated both the nature of the cycles under study and business-cycle theory in general. In this regard his work is a helpful contribution to both the historian and the economist. Basically historical in its approach, the book leans heavily on the theoretical foundations of business-cycle theory.

The book is divided, roughly, into two parts: the first part deals mainly with the theoretical framework and tools to be used; the second part contains historical accounts and analysis of the particular cycles in the period under review. After making his case for the historical approach rather than the inductive, deductive, or econometric, Fels painstakingly synthesizes a theoretical framework of eclectic quality, drawing mainly on Schumpeterian theory but including important additions from Hicks and R. A. Gordon. This framework is filled out by taking into account certain other significant elements in the business cycle. price and wage flexibility, long-wave depression, and the role of external events. The result is a set of theoretical benchmarks which help greatly to further understanding of each cycle and of the underlying forces which transcended the particular cycle.

In the second part of the book Fels goes through a historical analysis of the main aspects of the various cycles starting with 1865: the cycles of 1865–79, 1879–85, 1885–88, 1888–91, 1891–94, 1894–97. These analyses of the various cycles, the turning points of which generally are National Bureau dates except where occasionally the author critically modifies the month of peak or trough, include an explanation of the major forces shaping the particular cycle within the context of the author's modified Schumpeterian system where appropriate. The explanation in some instances departs widely from the Schumpeterian model in which case the author develops it in accord with Gordon's investment opportunity concept or brings in Hicksian concepts or a combination. Fels' explanations are often a happy intertwining of contemporary accounts and judgments with the most important statistical series which measure more concretely the dimensions of the fluctuation. In some instances he accepts uncritically the validity of contemporary judgments, and his notion that the depression of the eighteen nineties was intensified by net outflow of capital from the United States is subject to serious questioning. But on the whole the account is balanced and penetrating. Constantly referring causal relationships to his theoretical framework, the author succeeds in putting each cycle into a clearer focus.

As the author takes the measure of the cycles from 1865 to 1897 he concludes that "on the one hand drastic modifications need be made in Schumpeter's theory, and on the other hand the model is exceedingly illuminating." Innovation, Juglars, and Kondratieffs all proved very useful concepts while Kitchins did not. Gordon's minor cycles were more useful than Schumpeter's Kitchins. Hicks' theory contributed least to the study of the cycles. Fels concludes further that the framework applicable to the 1865–97 period seems to hold valid for the 1945–58 period. Regardless of the validity of this, Fels' work does provide a solid historical and statistical study with much material enhancing our understanding of both economic history and business-cycle theory.

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DOUGLAS VICKERS. *Studies in the Theory of Money, 1690–1776.* Pp. ix, 313 Philadelphia: Chilton Company, 1959. $6.50.

This book is based on a doctoral thesis presented at the University of London. It re-examines economic literature in this period to elucidate the theories presented on money, employment, and to some extent on interest and economic growth Successive chapters deal with Locke, Barbon, North, Law, Berkeley, Vanderlint, Cantillon, Hume, and Steuart. In general, the view is taken that there was more "wholeness" in the ideas of the later mercantilist and transitional writers whom Vickers studies than often has been recognized. Vickers makes it appear that in substantial degree these men were macroeconomists of modern type, concerned particularly with full employment. He marshals his evidence to build a good case and is to be congratulated on it.

It is generally conceded that mercantilism was a "macro" system so far as it was an economic system at all. The mercantilists professed to look at the economy in a broad way and expected the government to run it properly. From Misselden onward, the argument was presented that more money meant more employment. It was suggested frequently that banks should be chartered to give loans and issue money:

why buy specie from abroad with goods exports when paper would raise prices just as well? This is where later writers parted company with the earlier mercantilists.

Vickers has done good service in bringing to notice aspects of the literature in his field that have received relatively little attention in standard histories of economic thought. However, his book suffers in some degree from the limitations of its scope and, partly because of this, seems sometimes to overstate its case. The omission of reference to Misselden, who wrote before Vickers' period, makes some ideas dealt with appear more novel than they really were, and Vickers' concentration on analysis of the writings to the neglect of their background perhaps puts Berkeley in a somewhat different position than he ought to occupy. Berkeley, in the *Querist,* was concerned with Irish poverty, which was a different and more lasting condition than the trade depressions that interested some of the English writers who made comparable suggestions. Vickers' statements that Barbon's ultimate criterion of policy was employment, and Law's that of power through employment, are difficult to accept without reservation. Barbon's *Discourse of Trade* suggested consumers' goods as an objective and Law probably was as much concerned as earlier mercantilists with wealth and social stability.

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W PAUL STRASSMANN. *Risk and Technological Innovation: American Manufacturing Methods During the Nineteenth Century.* Pp x, 249. Ithaca, N. Y: Cornell University Press, 1959. $4.00.

Inspired by the speculation of economists, especially Schumpeter and Veblen, on the impact of innovations on our economy, the author in this study makes an empirical examination of segments of American industry in the nineteenth century, endeavoring to determine the relationship among business enterprise, technological change, and the risks assumed by entrepreneurs in fostering the innovations that were undertaken. He studies the history of the production of iron and steel, textile machinery, machine tools, and electric power equipment to find out when innovations took place, the entrepreneural problems and decisions that led to these developments, and the degree of risk encountered by the promoters of new manufacturing methods in these fields.

Strassmann defines innovation as the time when a first decision is made to alter the routine of production and funds are used to implement a new, untried technological change. His investigations disclose that innovators were more creative and less passive than Veblen thought them to be, but that they proceeded with a larger degree of caution as well as a more certain chance of success than would the speculating gamblers that Schumpeter described as innovators.

In the iron and steel industry, most early mill owners were not enthusiastic about experimenting with technological changes, and simply followed Europe's lead. After 1870, however, American entrepreneurs became less cautious and at last effected technical improvements. In the textile industry, Europe again took the lead. The expense of changing heavy machines made Americans overly cautious and limited most innovations to attachments and gradual increases in speed The low risks in the machine tool field, because the cost of these tools was not great, permitted considerable innovation, but the number of innovating firms was small, most enterprisers preferring merely to copy. Caution and the experience necessary in the electric power industry limited the field to a very few firms, most of which, however, made many successful innovations.

To this reviewer, the book seemed disappointing. It closely resembles a Ph D. thesis in format and confirms the oft-made observation that doctoral dissertations do not normally make good books. In addition, the constant references to the technical processes and equipment used in the industries studied makes it quite difficult for the average reader to follow the discussion. Finally, the limitation of the investigation to only four industries, all involved with heavy producers goods, makes the validity of the thesis questionable for any generalizations to be applied to the entire economy.

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Calvin B. Hoover. *The Economy, Liberty and the State.* Pp. 445. New York: Twentieth Century Fund, 1959. $5.00.

The events of the first half of the twentieth century—two world wars, the rise of communism, important modifications in political and economic institutions in the countries of classical capitalism, fantastic technological and scientific advances—initiated a new period in the history of mankind.

Dr. Hoover has undertaken the task of presenting the development, during several recent decades, of countries with different socio-economic structures, and of showing how their development has affected personal freedoms. He discusses the possibility of the rebirth of freedom in the "peoples' democracies" and the possibility of maintaining freedom in the capitalist countries, under the assumption of uninterrupted evolution of their socio-economic structures.

The book consists of eleven chapters, excluding the Prologue and the two concluding chapters. The first chapter is entitled "Old-Style Capitalism," four chapters are devoted to the Soviet Union, one chapter discusses "The Nazi Totalitarian System," three chapters analyze the economic system of the United States and two chapters cover "The British Economic System" and "The Economic System of Western Europe."

In probing this theme, Dr. Hoover also discusses a number of other questions closely related to the freedom of the individual in countries with contrasting socio-economic structures.

Lack of space does not permit a more detailed description of the contents of this valuable book, saturated with quantitative data, examples, facts, and the author's personal observations. Dr. Hoover is a leading economist and an expert on Soviet practices. From the reviewer's point of view the most interesting chapters are devoted to the Soviet political and economic system. The author presents many ideas, facts, observations, illustrations, and comparisons pertaining to the Soviet economic system, which make the book valuable to a specialist and interesting to a layman.

It is important to note that the author sees the Soviet economy as it is in actuality, and not as we would wish to see it or as the Russians would like us to see it. Although the author knows well the shortcomings of the Soviet economic system, he was not lulled by these deficiencies into underestimating the Soviet potential. It cannot be said that Dr. Hoover did not see the forest for the trees. He stresses that "a Soviet rate of economic growth almost twice the past rate in the United States, if the writer's crude estimates were assumed to present reality, would represent a factor of great force in the present world conflict" (p. 164).

It is necessary to note an unfortunate inaccuracy in the book. Discussing the 1932–1933 famine in the Ukraine, the author writes: "Since the season turned out to be a poor crop year in the Ukraine, the harvest proved insufficient to maintain life. The peasants appealed for grain from the state grain reserves. Stalin said 'No!' Hundreds of thousands in the Ukraine died from starvation or diseases of malnutrition" (p. 73). Actually the situation was somewhat different. Famine was quite deliberately employed by the Soviet government as an instrument of national policy, as a last means of breaking the resistance of the peasantry to the new system where they are divorced from personal ownership of the land and obliged to work under the conditions which the state may dictate to them and deliver whatever the state may demand from them.

According to estimates of various authors, between three and five million people died of hunger in the Ukrainian Soviet Socialist Republic at that time. Famine, as a political tool, is the cruelest invention of the communist system

Although Dr. Hoover's book will be especially useful for those who are working in the Soviet field, it is wholeheartedly recommended to all who are interested in and take to heart the more disturbing problems of our times.

T. SOSNOVY

Specialist in Soviet Economy
The Library of Congress

J. FRED RIPPY. *British Investments in Latin America, 1822–1949, A Case Study in the Operations of Private Enterprise in Retarded Regions.* Pp. xii, 249. Minneapolis: University of Minnesota Press, 1959. $5.00.

Dr. J. Fred Rippy, who has dedicated many years to the study of Latin America, in the volume under review has presented an interesting and important survey of a special phase of the economic development of that region. It is in fact a case study in the operation of private enterprise in retarded regions. It considers from all possible angles the operations of the British investors in Latin America from the era of independence until 1949. The author has investigated carefully all the evidence available regarding the chronicle of private English investments in Latin America, and directs attention to the unequal economic progress among the various nations and peoples. Details of the size, nature, chronology, and rates of return of the British investments are presented.

The introductory chapter deals with recent problems in private international investment and details the crisis which has occurred in the British experience in Latin America. Then three parts are devoted to the various phases of the problems involved Part one comprises a general survey of the British investments in Latin America. The various periods into which the investments fall are indicated, showing great activity at times and much reluctance at others. There are chapters devoted to a detailed analysis of investments at the end of 1913, British investments at their peak in 1928, and a decade of rapid contraction which closes the period covered to 1949.

Part two is devoted to a study of the British investments made in the various countries of Latin America. The importance and results are set forth. The amounts invested, the returns obtained by the investors, and the importance of the development in each country are indicated. Part three comprises chapters dealing with the nature and results of certain British companies and the income from British overseas investment during a recent decade. It concludes with chapters on some British views of foreign investments and the view of the Latin American recipients.

Many statistical tables are included and the Appendices list the British gas companies in Latin America and the British investments in Latin American electric utilities. Thus it can be said that Dr. Rippy has made a valuable contribution to the study of certain aspects of the economic development of Latin America during the period of its independence. The volume should be read by all who desire to understand the course of events in the Latin American countries.

ROSCOE R. HILL

Washington, D. C.

Wages, Prices, Profits and Productivity. (Background Papers and the Final Report of the Fifteenth American Assembly, Arden House, Harrison Campus of Columbia University.) Pp. ix, 193. New York: American Assembly, Columbia University, 1959. No price.

The relationship among wages, prices, profits, and productivity was the subject for discussion at the Fifteenth American Assembly, Arden House, Harriman, New York, April 30 through May 3, 1959, under the auspices of Columbia University. Some sixty Americans from business, labor, government, agriculture, communications, and the clerical, legal, and military professions participated.

Special papers by a group of distinguished economists provided background material for the conference. Professor Charles A. Myers, Massachusetts Institute of Technology, edited the papers and contributed the Introduction titled "Central Issues in Wage Price Relationships." The papers, which constitute the major part of the present volume, include: "Patterns of Wages, Prices and Productivity," Professor Albert Rees, University of Chicago; "Productivity, Costs and Prices," Professor John W. Kendrick, George Washington University; "Underlying Factors in Postwar Inflation," Professor James S. Dusenberry, Harvard University; "The Impact of Unions on the Level of Wages," President Clark Kerr, University of California; "Wage Behavior and Inflation," Professor Lloyd G. Reynolds, Yale University; "Policy Problems," Professor John T. Dunlop, Harvard University.

Also included in the volume are an address by Secretary of Labor James P. Mitchell on "Productivity and the Consumer," an address by the late Professor Sumner H. Slichter of Harvard University on "Labor Costs and Prices," and the "Final Report of the Assembly."

Analysis of the relationships among wages, prices, profits, and productivity indicates the need for more complete data and more accurate measurement. Considerable attention is given to the relation of wages to inflation While wages are seen as an important contributing cause, the consensus is that in addition to the upward push of wages and other costs it is essential to recognize the impact of the upward pull of demand resulting from such factors as the backlog of purchasing power, easy credit, heavy public expenditures, and increasing population.

Viewed as desirable national goals are: sustained economic growth, increased productivity, full employment, reasonable price stability, national security, industrial peace, and a higher standard of living. Illustrative of the means of attaining these goals are: appropriate monetary policy; free markets; reduction of tariffs and other impediments to international trade; vigorous enforcement of anti-trust laws; and an annual conference of labor, management, and government to examine the trend of wages, prices, profits, and productivity in relation to national objectives.

Comprising as it does a series of brilliant analyses of the diverse facets of inflation and the elements of sound public policy, this volume is a very important addition to contemporary economic literature.

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RICHARD HARTSHORNE. *Perspective on the Nature of Geography.* (The Monograph Series of the Association of American Geographers.) Pp. 201. Chicago: Rand McNally & Company for the Association, 1959. $5.00.

This book is both a summary and elaboration of Professor Hartshorne's previous methodological work entitled, *The Nature of Geography,* published during 1939 in the *Annals* of the Association of American Geographers. In 1946 the original monograph was reprinted in book form with minor additions and alterations by the author. American geographers were, and still are, given to frequent discussions of the nature and scope of their field. As the most comprehensive analysis of the subject in English, the monograph helped to give us perspective in the light of European concepts, particularly those of Ritter, Humboldt, Ratzel, and Hettner, and became the credo of the postwar generation of graduate students in Anglo-America.

Although the new book cannot replace the earlier work because it does not duplicate the exhaustive treatment contained therein, it expresses briefly and in a more affirmative manner a logical framework of geographic investigation and scholarship. It brings together references to subsequent writings in the English, French, and German languages, and takes cognizance of more recent thinking. In view of the present rapprochement between Russian and Western scholars and scientists, the paucity of references to the literature in Slavic languages and the lack of discussion of Russian geography might be considered a deficiency.

Consideration is given to ten basic questions, each of which forms a theme of a chapter: "What Is Meant by Geography as the Study of Areal Differentiation?," "What Is Meant by the 'Earth Surface'?," "Is the Integration of Heterogeneous Phenomena a Peculiarity of Geography?," "What Is the Measure of 'Significance' in Geography?," "Must We Distinguish between Human and Natural Factors?," "The Dualism of Physical and Human Geography," "Time and Genesis in Geography," "Is Geography Divided between 'Systematic' and 'Regional' Geography?," "Does Geography Seek to Formulate Scientific Laws or to Describe Individual Cases?," and "The Place of Geography in a Classification of the Sciences."

In this, as in the earlier work, the author's endeavor is to report and document the concepts of the field found valid by others in Europe and America. Yet I detect a somewhat stronger expression of his personal views, particularly in regard to the place of genetic geomorphology (pp. 93–96), which is likely to arouse adverse criticism on both sides of the Atlantic. Anyone who seeks in this volume a set of final conclusions to end all arguments will be disappointed. No methodology should be considered complete.

Few will doubt, however, that many fundamental issues have been resolved and many illusions dispelled, including the notion that geography is an offshoot of geology despite the nurturing it received from geology in American colleges and universities during the early twentieth century The value of Hartshorne's labor will be evident if it promotes general agreement among geographers and releases their energies for substantive studies built in an organized system.

JOHN E. BRUSH

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F. ERNEST JOHNSON and J. EMORY ACKERMAN. *The Church as Employer, Money Raiser, and Investor.* (Series on the Economics and Ethics of Society.) Pp. xvi, 184. New York· Harper & Brothers, 1959. $4.00.

Two competent authorities, clergymen skilled in social research techniques including questionnaires and statistical tabulations, present a reasonable although not exhaustive interpretation of the relation of American Protestant churches to the current economic picture. They ask: do religious groups live up to the accepted standards of corporate practice, and should they apply even stricter standards to themselves? Often the evaluation favors the churches; lags and inconsistencies seem due to inadequate funds or lack of acquaintance with business and financial techniques.

Investigation reveals that some nine well-established denominations have higher salaries than do the smaller sects. Including allowances for house rent and car, three denominations average more than $6,000; six others about one thousand dollars less. As a rule, the larger the community and plant, the higher the salary. Even the best averages are judged inadequate for the proper support of a family with children. Major agencies, like national denominational boards and the National Council of Churches, have better salary scales and fringe benefits. Pension systems, re-inforced by social security, although insufficient are improving. The income of pastors is higher than that of directors of religious education, parish workers, office secretaries, and sextons, whose remuneration drops in that sequence.

The securing of funds, with or without the aid of professional money-raising organizations, is too commonly based upon excessive promises and directed toward givers motivated by forces other than purely dedicated stewardship. Ownership and investment practices approximate those of the business world, at times applied skillfully through the counsel and service of devoted laymen, but frequently based upon expediency rather than principle.

The authors consider that conditions are improving in regard to self-examination and effectiveness. They suggest further application of approved personnel practices such as job classification, racial integration, equal pay for women, and better provisions for illness, health, and vacations. They conclude with the caveat that the more the churches succeed in the realm of business and finance, the more they may take on the color of their secular environment. It would seem then to be the concern of sensitive Christians to lead rather than conform or follow, to strengthen the spiritual power of the churches, and to make their character and mission more visibly moral and socially beneficial

EARL CRANSTON

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AMERICAN GOVERNMENT AND HISTORY

MAX BELOFF. *The American Federal Government.* Pp. vii, 213. New York: Oxford University Press, 1959. $4.50.

Profesor Beloff set himself "in this book only a limited task: to give an account of the general principles of American government and of the manner in which it functions from the point of view of non-Americans, whose main concern is naturally with those aspects of American institutions and policies that affect the outside world." His viewpoint is necessarily that of the British subject, firmly committed to "responsible government" by a cabinet accountable to parliament. His critique of American government is couched in British terms. It might have started a debate about the relative merits of the two systems, if the late Harold J. Laski and Don K. Price had not pre-empted that field.

The book seems to have been meant as another in the series of European commentaries upon the United States, to which the contributions of De Tocqueville and Bryce have been the most distinguished ones. It impressed this reviewer more as a primer on American government for literate Britons. The core chapters deal with the Constitution, the President, the Administration, the Congress, and the parties. They do so at equal length, except for the Administration, to which more space and criticisms are offered. Mr. Beloff is especially unhappy about the Civil and Foreign Services. The latter "is forced in its recruitment to accept Congress' prejudices in favor of a spread by region, which may not necessarily correspond to the distribution of the universities and colleges most likely to turn out the best candidates." This overlooks the nationwide recruitment of students by these universities. And it is simply not true "that the highest posts are barred to the career man. . . ."

The conclusion favors constitutional reform Of this, Mr. Beloff writes: "Where those relatively qualified to speak have been silent, it is not for the foreign student to make himself heard." Before the reader can applaud this thought, he continues: "But he cannot help seeing the United States as the extreme case of the common fate of modern governments in being unable to cope effectively with the ever growing burden of day-to-day responsibilities, and in being forced to postpone or neglect important decisions of principle, including the shape of government itself." The merit of this book is directly related to the irrelevance of the conclusion to its core. However, on the whole, it presents an informed and balanced view of American federal government.

HERBERT JOHN SPIRO
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RICHARD F. FENNO, JR. *The President's Cabinet: An Analysis in the Period from Wilson to Eisenhower.* (Harvard Political Studies.) Pp. xii, 327. Cambridge, Mass.: Harvard University Press under the direction of the Department of Government, 1959. $5.50.

Several approaches might be followed in studying as unmanageable and desultory an institution as the President's cabinet. A purely chronological one would be of interest to the historian, a purely analytical one to the political scientist. Mr. Fenno, in his study of that body from Wilson to Eisenhower, has managed to incorporate both with results of interest to both and by keeping his style relatively free from professional jargon has provided a study of value to anyone interested in American politics.

Beginning with a discussion of cabinets in general, "A cabinet originates in the universal need on the part of any single Chief Executive to consult with others and draw upon the advice of others in exercising his political power. It persists as an aid to intelligent and informed decision making," Mr. Fenno proceeds to analyze the appointment process, cabinet meetings, intercabinet and interdepartmental co-operation during and outside of formal meeting, the relation of the cabinet to the President, Congress, political parties, and pressure groups and concludes with a contemplative discussion of a series of proposals for and attempts at cabinet reform.

In all instances, he draws on examples over the last forty-five years for generalizations and conclusions.

And his conclusions are sane and perceptive. Generally they are that the cabinet is and ought to be an agency flexible enough to serve the best interests of the President in a variety of fashions. This, however, makes specific obligations vague, and Mr. Fenno makes clear that those obligations are not fixed or formal and can and have changed with individual Presidents and diverse cabinet personalities. Certain Presidents, for example, have expected their cabinets to serve as policy-making bodies. Harding felt his team of "best minds" could make up for his own inadequacies and chart an intelligent course for the nation. Hoover, on the other hand, made his own decisions, and Franklin Roosevelt used advice when it suited his general purposes and rejected it when it did not. But policy making is only one function. The cabinet is also a vital sounding board of public opinion, with different members responding to different sources of such opinion, and a popular member, popular that is with his particular "constituency," often serves admirably to draw support to the President. Here however lies a paradox. To be most useful the member must have non-Presidential "public" prestige, party following, legislative support, or roots of influence in his own department. And this means the member operates, not as part of the formal body but independently, and frequently means that he operates to get for his supporters the bigger piece of the pie before a fellow member moves in to do the same for his. In this sense the political help which the President receives comes not from the group but from individual members operating quite outside the group and often with ferocious individualism.

However, although Mr. Fenno discusses at length the problems of "departmentalism," he does not decry it too vigorously. On the contrary, he generally takes a dim view of proposals to formalize and institutionalize the cabinet and force it to work as a team. In this regard his discussion of the Eisenhower Secretariat approach is of interest. However, again, his analysis of this experiment is properly detached. In some respects it has worked for Eisenhower, yet, as Mr. Fenno states: "one should remain reluctant to prescribe it or any other specific reform as the solution to the President's problems, lest a rigid prescription or an over-institutionalization of the means defeat the very ends of executive leadership"

In all, then, the cabinet is a vital part of American political life, not perfect nor even valuable in many instances but potentially very useful and well worth keeping in its "unreformed" status for just this reason.

PAUL L. MURPHY
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LEWIS MAYERS. *Shall We Amend the Fifth Amendment?* Pp. x, 341. New York: Harper & Brothers, 1959. $5.00.

Until the last decade, the Fifth Amendment to the federal Constitution was chiefly known for its double jeopardy and due process clauses. Buried in its verbiage was a fifteen-word phrase—"No person . . . shall be compelled in a criminal case to be a witness against himself"—about which most Americans then knew little or nothing. Yet today there is probably no constitutional provision that is more notorious than this so-called "privilege against self-incrimination."

Now Professor Lewis Mayers, long a fixture in the law department of New York's City College, has joined the growing ranks of the Fifth Amendment's detractors. This book, *Shall We Amend the Fifth Amendment?*, is the last of three significant recent books on the privilege. The first two, Dean Erwin Griswold's *The Fifth Amendment Today* and Professor Sidney Hook's *Common Sense and the Fifth Amendment*, were clearly inspired by the use or misuse of the privilege before congressional investigating committees and were mainly concerned with the inferences to be drawn from its invocation. Curiously enough, Professor Mayers' critical book, which is incomparably more comprehensive than its predecessors, not only defends the right of all witnesses except public officers to plead the Fifth Amendment before these committees but even recommends its further liberalization. His opposition stems from what he considers its harmful effects insofar as law enforcement is concerned, an area in which legislative committees play little part.

His attack on the privilege is on two levels: its use by criminal suspects during preliminary interrogations by police, magistrates, and grand juries; and by nonimplicated witnesses in the trials themselves. In both cases, he advocates its outright abolition provided the prosecution is prohibited from offering the compelled testimony in evidence at any subsequent trial of the witness in question. He urges this approach to the abrogation of a constitutional right as a proper balance between the needs of law enforcement on the one hand and the individual's civil liberties on the other.

It is a constitutional necessity that even sacrosanct First Amendment rights must occasionally yield to compelling necessity. But Professor Mayers has not convinced me that lawlessness has reached the point where it can only be controlled by the destruction of fundamental rights. And the abolition of the privilege cannot be justified by the granting of a limited immunity which can never be as broad as the right it replaces. All he offers is the proposal that the compelled testimony cannot be used later against the witness. But there is nothing to prevent the prosecutor from utilizing the information gleaned from it to build a substantial case against the testifier. Mayers' suggestion is clearly unconstitutional, and as he somewhat querulously points out, today's Supreme Court would quickly invalidate any federal or state legislation incorporating his panacea.

It is not easy to sit still when Dave Beck stands on his constitutional rights and refuses to concede the paternity of his own son. Admittedly, it is exasperating, time-consuming, costly, and often even fruitless to force those responsible for the administration of justice to rely on secondary evidence because material witnesses sit smugly silent. But it is still better, even at the expense of some dead-end investigations or unfortunate acquittals, to retain these differences in coloration that mark us from the totalitarian states. Jerome Frank saw this more clearly than Professor Mayers seems to when he wrote "Before we . . . sacrifice all our other values to effective law enforcement, we should reflect on the brutal consequences of the totalitarians' alleged efficiency in pursuing suspected criminals."

WILLIAM M. KUNSTLER
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RICHARD H. LEACH and REDDING S. SUGG, JR. *The Administration of Interstate Compacts.* Pp. vi, 256. Baton Rouge: Louisiana State University Press, 1959. $4.50.

This book breaks new ground in the field of public administration, being the first comprehensive attempt at an appraisal of the organization, staffing, and practices of interstate compact agencies. It is the outgrowth of a project begun while the authors were staff members of the Southern Regional Education Board, an interstate compact agency established in 1949 and operating with notable success in the southern states from Maryland to Texas.

The authors have produced a well-written and readable book, without excessive details or extraneous matter. They have summarized the development of interstate agencies under the compact clause, discussed the relations of interstate compact agencies with state and federal governments, analyzed the powers and personnel of interstate bodies, and treated the staffing and operations of these bodies at some length. They have selected six interstate agencies—the Pecos River Commission, the Atlantic States Marine Fisheries Commission, the Ohio River Valley Water Sanitation Commission, the Waterfront Commission of New York Harbor, the Interstate Oil Compact Commission, and the Southern Regional Education Board—for special case studies. Finally, they have added some interesting conclusions and predictions about interstate compact agencies, and they have commented helpfully in a bibliographical note on available reports and other materials.

In discussing the development of interstate compact agencies, the authors point out that these agencies have been used to any extent during the past forty years only, more particularly since World War II, that they now provide "a new dimension for state power," and that "they permit the states to take continuing cooperative action in fields where they cannot act effectively or do not wish to act alone, fields which might fall by default to the federal power if not occupied through the initiative of the states." Of more than sixty interstate compact agencies established in recent years, thirty have set up administrative facilities to carry on their work and consequently are the ones which the authors have chosen to discuss. In all cases, these compact agencies are identified administratively with the states they represent rather than with the federal government.

While the idea of interstate compacts is as old as the federal constitution, the authors observe that the use of such compacts is still too new for any well-defined relationship to have developed between the states involved and the compact agencies themselves. But they think that the point has been reached where it is apparent now that the compact agencies can do new kinds of work hitherto inhibited to the states on account of "their adventitious physical and administrative boundaries." And the success of such agencies, they find, depends directly upon the ability and continuity of the staffs recruited by these agencies to carry on their work.

Sparked by the general attitude of the Eisenhower administration and the findings of the Kestnbaum Commission, there is currently renewed emphasis on the position and powers of the states in the federal union. So the authors conclude that "interstate compact agencies can play a tremendous role in the states' resumption of power."

A. E. BUCK

Norwalk, Connecticut

MRS. THEODORE ROOSEVELT, JR. *Day Before Yesterday: The Reminiscences of Mrs. Theodore Roosevelt, Jr.* Pp. 478. Garden City, N. Y.: Doubleday & Company, 1959. $5.95.

Although Mrs. Theodore Roosevelt, Jr., nee Eleanor Alexander, has previously been largely unknown to students of the American scene, she should now gain her richly deserved place as one of America's great ladies of the twentieth century. This book, which is, first of all, a generous, unselfish autobiography, approaches the status of a biography of Theodore Roosevelt, Jr., the author's distinguished husband. Here for the first time are revealed many interesting anecdotes of the "off season" Roosevelts, as they are termed.

Theodore Roosevelt is mentioned, but this is not a book about him. Nor does it treat to any appreciable degree the life of Mrs. Theodore Roosevelt or of the author's well-known sister-in-law, Alice Roosevelt Indeed, when the author ushers her in-laws into her fascinating narrative, it is the brothers of her husband who are introduced. Most of the information about Theodore Roosevelt, Jr.'s brothers is connected with their gallant records in military service during World War I. From her position as an employee of the Young Men's Christian Association (Y.M.C.A.) in France in 1917–1918, Mrs. Theodore Roosevelt, Jr. has told of a most unusual segment of her life.

This book gives the American public an excellent example of how partisan politics sometimes intervenes in war activity. Woodrow Wilson refused to permit Theodore Roosevelt to lead a division to France during World War I. That fact has been widely known but not so well known was the order issued during World War II, apparently by the Commander-in-Chief, Franklin Roosevelt, that in all reports of heroic feats performed by Theodore Roosevelt, Jr. or by his son, Quentin, the name of the Roosevelt in charge was to be deleted from all information sent to the United States. Although both father and son were recognized by foreign governments for meritorious service, not one word of this heroic military action was known in America. Only after Theodore Roosevelt died in France in July, 1944, was publicity given by his country to his distinguished military career during World War II. He was awarded the Congressional Medal of Honor posthumously.

Notice of

ANNUAL BUSINESS MEETING

of the members of The American Academy of Political and Social Science

In accordance with the by-laws of the Academy, the Annual Business Meeting of the members will be held on Monday, February 8, 1960, at 2:00 P.M., at the offices of the Academy, 3937 Chestnut Street, Philadelphia, Pa.

The Annual Report of the Board of Directors will be presented, directors elected, and other important business transacted.

James C. Charlesworth
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The general topic will be announced in the next volume of THE ANNALS

James C. Charlesworth
President

Not all of the author's life, nor that of her husband, was consumed in military action. For several years Theodore was Governor of Puerto Rico and following that he was Governor General of the Philippine Islands. In both areas, the author reports the career of her husband as an efficient colonial administrator. She also weaves into her story much interesting material about herself and her children.

Another significant phase of Mrs. Roosevelt's life was spent in accompanying her husband to Asia on hunting trips for the Field Museum of Natural History. This accomplished lady proved herself to be an excellent rifle shot and a good companion on a safari.

From helping to organize the American Legion in 1919, Theodore entered politics. As Assistant Secretary of the Navy in 1921, he played a significant role in the Washington Disarmament Conference. Although Roosevelt was almost brushed by the Tea Pot Dome Scandal, he emerged without stain to run for governor of New York in 1924. Defeated by Al Smith, Theodore, assisted by his devoted wife, found another outlet for his boundless energy.

This well-written and informative book will be read with delight by many people. It records the life of a warm and wonderful family. At the same time, it evokes the history of the last half-century with great grace and charm.

GEORGE C. OSBORN
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University of Florida

HARRY V. JAFFA. *Crisis of the House Divided: An Interpretation of the Issues in the Lincoln-Douglas Debates.* Pp. 451. Garden City, N. Y.: Doubleday & Company, 1959. $6.50.

Professor Harry Jaffa, a political scientist, has written an important book about one of the great episodes in the history of the sectional controversy, the Lincoln-Douglas debates. To evaluate or even to discuss the book is a difficult task. It is a deeply thoughtful work and at times a tedious one. It is at once profoundly philosophical and ponderously pretentious. It is also a very angry book The author lashes out constantly against the views of such writers as Avery Craven, Richard Hofstadter, George Fort Milton, Allan Nevins, Charles W. Ramsdell, J. G. Randall, and T. Harry Williams. This is a distinguished list, but Professor Jaffa has every right to differ with these scholars; indeed, on many scores his criticisms are justified. But like so many writers who want to prove that everybody else is wrong, he sometimes gets carried away by the exuberance of his opinions and voices too extreme statements.

The principal targets of Professor Jaffa's thrusts are the so-called 'revisionists" and particularly the leader of that school, J. G. Randall. In brief, the revisionist view was that the Civil War was not the product of slavery or of any fundamental cause, that it was artificial and brought about by a fanaticism, mainly Northern in origin, that ignored realities, and that it could have been avoided if the politicians had acted more responsibly. It so happens that I agree with many of Professor Jaffa's strictures on the revisionists He has performed a service by calling attention, even if too bitterly, to their errors. But he does not see that the revisionists were influenced by the intellectual currents of their time, as all historians are, as he himself is influenced by certain concepts of our own day. He rightly finds more principle, more idealism, more consistency in the antislavery movement than previous writers—but he finds too much.

Professor Jaffa's thesis is too complex to explain in a short space. Briefly, he examines Lincoln's early thought, especially as expressed in the Lyceum and Temperance addresses, and finds therein a depth and duality not previously appreciated. In expounding Lincoln's arguments in the famous debates, he properly emphasizes Lincoln's devotion to the principle of equality, and he has some sensible things to say about the meaning of equality in the American ethic. This part of the book is excellent and should be read and pondered by every serious student, regardless of whether he agrees with the author's argument. It breaks new ground and opens a new view of Lincoln's significance as a political thinker.

Of one of Professor Jaffa's criticisms of historical writing, I can speak with direct knowledge. He attacks me for saying that Lincoln held the Negro to be inferior and that the superior whites would always oppress the blacks. On the first point, he complains that I do not specify in what way Lincoln thought the Negro was inferior or how long the inferiority would last. Lincoln himself probably could not have defined his feelings on this issue exactly. On the second, Professor Jaffa argues that Lincoln said only that one race or the other would do the oppressing. It is obvious, I think, that Lincoln felt if any oppressing was done the whites would do it. Of course, he did not say, nor did I so report him, that one race would always oppress the other; he was talking about prospects in the foreseeable future. Professor Jaffa is correct in saying that Lincoln's racial ideas have been the subject of some "loose" scholarship. They may now become the victim of some dogmatic moralizing.

T. HARRY WILLIAMS
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JOHN R. CUNEO. *Robert Rogers of the Rangers*. Pp. xii, 308. New York: Oxford University Press, 1959. $6.00.

Few figures of any real importance in our eighteenth century have been so ignored by historians as has Robert Rogers of the Rangers. This does not mean to say that he is unknown. Kenneth Roberts' novel, with the resultant movie and television programs, have made "Rogers of the Rangers" almost a household word. The reputation of Robert Rogers has been swollen by legend and festered by prejudice. Until the present volume, however, there has been almost no attempt at serious and thorough research into the facts behind the legend.

That Robert Rogers deserves the attention of a serious and competent biographer cannot be challenged. Rogers volunteered to fight against the Indians at fourteen; at twenty-four he became a scout for Sir William Johnson, on the shores of Lake George. There followed many gallant forays against the French and Indians; the dramatic destruction of the St. Francis Indians; the generally unsuccessful but glamorous expeditions into the West. Rogers won generous and powerful support in England, wrote and published volumes that were critically acclaimed, organized and commanded battalions of scouts and rangers, demonstrated the possession of military intelligence and insight of a high order, and married a beautiful and socially prominent girl.

Rogers also failed in business, contracted heavy debts, provoked the bitter animosity of such important men as Sir William Johnson and General Gage. Unjustly and maliciously accused of treason, he was acquitted only to find his career blasted. Years in debtors' prison were followed by permanent separation from his family, a lack of conviction during the American Revolution, and a final decade or more in England that was marked by debauchery and failure. As a gallant figure in the final English wars for possession of North America, as an untutored New Hampshire farm boy who demonstrated great courage, leadership, and military insight but almost no ability to adjust to civilian life, Rogers warrants attention, analysis, and understanding.

Mr. Cuneo, a Connecticut lawyer, has devoted many years and amazing perseverance to the difficult task of separating myth from fact and of identifying motives. His Chapter Notes are provocative and inspire confidence, and complete documentation, we are told, is available. His style is lucid and even brilliant, and his understanding of the complex Rogers and his frequently confused and complicated background, is unusual. Some scholars may feel that Mr. Cuneo has been too severe on Johnson and Gage; the fact remains that for the first time a brave and colorful soldier has found an adequate interpreter.

RALPH ADAMS BROWN
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EUROPE

WILLIAM DIEBOLD, JR. *The Schuman Plan: A Study in Economic Cooperation 1950–1959.* Pp. xviii, 750 New York: Frederick A. Praeger for the Council on Foreign Relations, 1959. $6.50.

An incredibly large literature has been produced on the European Coal and Steel Community (ECSC) since its formation in 1951. However, it is gratifying to say that the stout volume under notice is not just another addition to the studies. It is a major scholarly work on the ECSC during its period of transition to one of the institutions of the wider European economic integration—the European Economic Community.

The Coal and Steel Community derives from the Schuman Plan, proposed in May 1950, which had both a political objective and an economic purpose: to make war between France and Germany "not merely unthinkable, but materially impossible" by placing their entire production of coal and steel under a common organization open to other countries and developing a free trade market for these two basic economic sectors. It was established by the Treaty of April 18, 1951, that integrated the coal and steel industries of Belgium, France, the Federal Republic of Germany, Italy, Luxembourg, and the Netherlands. Mr. William Diebold, Jr., who is Director of Economic Studies at the Council on Foreign Relations, here presents the most substantial appraisal of the achievements of the ECSC and of the problems that it still faces that has so far appeared. The product of seven years of intensive research in documentary materials and interviews with leading Community officials and industrialists in Europe and the United States, it is likely to become the standard text on the subject in the English language.

In the four parts into which the book is divided Diebold discusses the background of the Schuman Plan and the developments that led to the ratification of the Treaty which came into force on July 25, 1952; examines the effort in the six member-countries to put the Treaty into practice by opening the common market for coal, iron ore, steel, and scrap among them and the Community's experience with transportation rates, subsidies, taxes, prices, investments, cartels, and labor; analyzes the relationships of the ECSC with external areas and American interests and policies toward the Community; and offers an evaluation of the institution in terms of "its economic impact, what it has meant to each participating country and to outsiders, the character of the coal and steel economy that has emerged, the kind of entity the Community has turned out to be, its relation to other measures of European integration and the consequences of 'partial integration.'" In a final chapter on "The Future of the Community" he attempts to clarify some questions that the ECSC will have to face and "to post a few markers by which the future course of the Community may be gauged."

The picture of the Coal and Steel Community which emerges from Diebold's elaborate study is that of a vital, functioning organization which has effected major accomplishments but has achieved only partially the task of integration. The common market for coal and steel is a reality. The Community has abolished barriers to trade in coal and steel; has progressively eliminated aids and subsidies and ended discrimination in prices and transportation rates; has subjected cartels, mergers, and investments to international regulation; has borrowed in the money markets of Europe and America where its credit is firmly established; and has developed new relations with governments, employers, and labor. Although problems of production, technology, and stability in the two sectors await solution, "The Community is now a part of the political and social furniture of Western Europe, one of the realities of its coal and steel economy." It "has changed Europe simply by existing. By surviving through its transitional period the Community proved itself something more than a good idea, a new hope, the latest expression of an ideal to which many had long paid homage. That so old a dream should have taken on institutional form, with organization, procedures, a daily routine of problems and activities, is itself the main fact about the Schuman Plan and a major statement about Europe of the 1950's."

ARTHUR LEON HORNIKER

Washington, D. C.

TIBOR MERAY. *Thirteen Days That Shook the Kremlin* Translated by Howard L. Katzander. Pp. vi, 290. New York: Frederick A. Praeger, 1959. $5.00.

Behind the dramatic cliché of the title there lies a serious and sober account of one of the most significant turning points of recent Central and Eastern European history. The subtitle of the book, "Imre Nagy and the Hungarian Revolution," must be closely considered by the interested reader since Meray's account is in reality a brilliant combination of the events of the short-lived Hungarian revolt of October–November 1956 and of a full-fledged biography of Mr. Nagy, Prime Minister of the country for those few exciting days.

Tibor Meray is obviously qualified for this delicate assignment. An outstanding writer and literary figure, he was once greatly favored by the communist regime of his native country, but later became a major protagonist in the revolution and finally was forced to flee to Paris where he is now living. His book is a microscopic—often far too detailed and technical for the nonspecialist reader—account of the bitter fighting within the ruling circles of the Hungarian Workers' party during the "New Course" period preceding the revolt. Imre Nagy emerges from these sordid and frightening struggles as an honest, serious, and sincere man, a lifelong communist with certain deep-seated convictions and scholarly interests. Unhappily he was also a clumsy tactician, an irresolute administrator, and a mediocre student of human nature. When forceful and aggressive leadership was required, Nagy missed the historical requirements of the moment, withdrew into himself, and by his irresolute bungling generally disappointed even his closest friends and collaborators

Meray's book has several shrewd and significant "asides," in addition to presenting a plastic and reliable portrayal of Imre Nagy. He reports that the strong United Nations resolution against the deportations of Hungarian youth had "succeeded in making an impression, the deportations having thereby been stopped" (p. 250). He also correctly stresses the steadily increasing influence of the Chinese Communist leadership in demanding a share of the decision-making process in the affairs of international communism. "The influence of Mao Tse-Tung and his friends had increased, their words carrying more and more weight" (p. 262 *et seq.*).

On the whole, although one might disagree with some of the author's editorial remarks and judgments of individual personalities on the Hungarian scene, he presents a valuable insight into Eastern European politics. This book will be indispensable for the student and the general reader alike.

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ISAAC DEUTSCHER. *The Prophet Unarmed, Trotsky: 1921–1929.* Pp. xii, 490 New York: Oxford University Press, 1959. $9.50.

With the present volume Mr. Deutscher has passed the halfway mark in an ambitious effort to tell the story of the Bolshevik Revolution in biographical-ideological histories of its principal actors: Stalin, Trotsky, and Lenin. He has completed his life of Stalin in two volumes. For Trotsky he is taking three, of which this is the second. Lenin is yet to come.

The first Trotsky volume, *The Prophet Armed,* carried the story from the latter's birth to that moment in 1921 when, as Commissar of War, he had just brought the Civil War to a victorious conclusion. At that moment he was at the height of his power and glory. The present work shows the "Prophet" unarmed, thwarted in his efforts to turn his talents to economic and political reconstruction, stripped of his badges of glory, and covered with badges of infamy and dishonor. It ends in 1929, with the "Prophet" expelled by the Party he helped to bring to power, deported by the government of which he was a founder, exiled from the land in which he was born to the limbo of the West.

The tragedy is told with great literary skill, with complete mastery of the documentary sources, always limited to Bolshevik documents, acquaintance with the intricacies of Bolshevik controversy, a deep understanding of most of the major and minor personages involved, with the notable exception of Bukharin, and, of course, no understanding for non-Bolsheviks. Since Mr. Deutscher was once an organized Trotskyist and is still an ardent admirer of his protagonist, he has shown an uncommon knowledge of what goes on within Trotsky's spirit.

Sometimes his admiration of Trotsky leads to uncritical absurdities. Thus this arrogant, dictatorial, and authoritarian man is declared to have been "almost equally sensitive to the claims of authority and freedom." He was sensitive only to authority as long as he exercised it, and to freedom—strictly limited to Communists—when he was forced into opposition. So, too, Mr. Deutscher proclaims his hero to have been "Russia's chief intellectual inspirer and literary critic in the twenties," then admiringly cites some monstrously cruel and insensitive remarks on poets like Blok, Gippius, and Biely, and his preference for "the very last louse on the most uncouth of our bagman-speculators" to the intellectuals who neither glorify nor oppose the regime but seek refuge in "in inner immigration" of their own thoughts and private creation.

The most interesting feature of the present volume is its ambivalence towards Trotsky's tragedy. Mr. Deutscher's admiration serves him well as biographer, but the ideological prepossessions he shares with both Trotsky and Stalin and, of course, with Lenin make him steer the tragedy towards a "happy ending." Defeated, expropriated of his program, slandered, expelled, deported, in the next volume to be murdered, yet . . . "Trotsky was the authentic inspirer and prompter of the second revolution of which Stalin was to be the practical manager." Now, under Khrushchev, according to the author, Trotsky is beginning to come into his own, a sort of macabre resurrection. "I see him as the representative figure of pre-Stalinist communism and the precursor of post-Stalinist communism." But the ruthless work of Trotsky's great and cruel antagonist was "the *sine qua non* for the ultimate fulfillment of the revolution's promise." It remains only to note that, for Deutscher, that promise includes not only a utopia in Russia but a winning of the world for the same blueprint, amended only by some touches derived from the benefit of hindsight and some traces of the utopian expectations of pre-Bolshevik Marxism.

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STANLEY W. PAGE. *Lenin and World Revolution*. Pp. xviii, 252. New York: New York University Press, 1959. $5.00.

Stanley Page's *Lenin and World Revolution* differs from most other studies of Lenin in the degree to which it emphasizes insatiable personal ambition as being the sole dominant motive of Lenin's whole political career. While this motive was rather unconvincingly disguised as political evangelism and care for the oppressed, it did not make him any less sincere a disciple of Karl Marx, whom indeed he closely resembled in the temporary use which he made of individuals, and whom he later disowned as soon as they started to endanger his determination to maintain supreme command.

Lenin's constant aspiration to international, rather than Russian, proletarian leadership, his seizure of power in 1917, his peculiar relations with Trotsky, his policy towards Germany during and after the Brest-Litovsk negotiations, his interest in stirring up the Asian countries, are all studied and revalued in the special light of Mr. Page's thesis.

There is a reasonable case for giving more encouragement to an approach which does not pander to the prevalent hero worship of temporary political success, built up by Lenin's admirers, still less to the myth that he embodied some inevitable forward movement of historical destiny. Lenin's concern for training chosen Asians to become anti-Western national socialists, under Russian control, is shown by his establishment in Moscow as early as 1920 of a so-called "Institute for Eastern Studies." These and other aspects of Lenin, referred to in this book, have not always received the attention which they deserve

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EDGAR MCINNIS. *The Atlantic Triangle and the Cold War.* Pp. viii, 163. Toronto: University of Toronto Press under the auspices of the Canadian Institute of International Affairs, 1959. $4.50.

Edgar McInnis has performed a remarkable service through his concisely and brilliantly written monograph, entitled: *The Atlantic Triangle and the Cold War.* According to the author, the Atlantic Triangle —United States, United Kingdom and Canada—is the "fundamental bone structure" of the western alliance, though he does not disparage the important roles played by France and West Germany.

Within the Atlantic Triangle, the United States must be the "leading and at times the decisive element," while both Britain and Canada recognize their "primary national interest is harmony" with the United States.

The author's thesis, very simply stated, is "to hold the line against the threat of aggression by force" and to "consolidate the unity of the non-communist world on a broad basis of social and economic advance." He is deeply concerned that the Atlantic Triangle has not, as yet, "evolved a global strategy" to effectively deal with "global interests."

McInnis, who is President of the Canadian Institute of International Affairs, advocates the basic requirement of military deterrence. Yet, he weakens another wise, sound military analysis by equivocation. On the one hand, he submits that "limited war remains a very real possibility," and, on the other side, declares that "both parties may be involved in a competition of increasing destructiveness whose end is unlimited war," thereby accepting the P. M. S. Blackett thesis.

Moreover, on the crucial German question, McInnis argues persuasively for the "continued presence of American troops" as a tangible assurance to our European Allies, and yet he suggests a policy of "disengagement" even to the point of acceptance of the Rapacki Plan.

In the economic arena, the author is on more solid ground, with the recommendation that the West must go beyond normal commercial contacts with the Afro-Asian nations and "become a major source of economic and technical aid." Yet, he is realist enough to advocate political strings by the West "as a lever to bring about the kind of [internal] changes that are needed if aid is to make the optimum contribution to economic and social improvement."

Several other pronouncements by McInnis lend interest to this authoritative little volume, such as the differences within the Atlantic Triangle over the Red China question and over the validity of the "northern tier" policy of the Bagdad Pact. All sober analysts, who are not fearful of facing the inevitable dilemmas that confront a coalition of nations, even though bound by a unity of purpose, should read this instructive monograph.

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HENRY PELLING. *The British Communist Party: A Historical Profile.* Pp. viii, 204. New York: The Macmillan Company, 1959. $3.75.

The British Communist party has known little but frustration and failure. In the late 1940's, Communist influence in some important British trade unions reached dangerous proportions, but it began to crumble as soon as the situation became generally recognized. Otherwise, the Communists' main achievements have been negative and unintentional: their existence provided an excuse for those who wanted to maintain the diplomatic isolation of the Soviet state in the 1920's; they also helped, by their clumsy attempts, to infiltrate the Labor party, to keep that party unshakably loyal to democratic practice and the parliamentary system even when, as in 1919–20, 1926, or 1931, there was some temptation to abandon these restraints.

Mr. Pelling's book is admirably straightforward and unpretentious. There is no attempt to inflate the feeble British party into something of universal importance. The author is well aware of the ironies, often grotesque, in which the party's history abounds. The vanguard of the proletariat achieved even the relative successes of the later 1930's only by enrolling a coterie of bourgeois intellectuals; as a party that was ever ready to sacrifice both principle and consistency for the sake of power it never managed to capture more than two parliamentary seats at a time. But Mr. Pelling wastes no time on preaching or polemic; the style throughout is economical and clear. Sometimes it is brilliantly succinct, as in the account of the Comintern's demise (pp. 121–124), or the paragraph about the self-deception of Communist intellectuals (p. 105).

A book of this size, written mainly from published sources and with an almost austere self-restraint, cannot answer all our questions about communism in Britain. To say that the party failed because "throughout the whole period, the British people have never been in a revolutionary mood" (p. 182), is perhaps not so much an explanation as it is a rephrasing of the central problem. But it would be unfair to charge Mr. Pelling with failing to provide the last word on his subject; this was plainly not his intention. *The British Communist Party: A Historical Profile* is a very useful book, the more so for being carefully and attractively produced at a comparatively sane price.

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LORD BEVERIDGE. *A Defence of Free Learning*. Pp. xiv, 146. New York: Oxford University Press, 1959. $2.90.

In March 1933, the author was in a Vienna cafe when the evening paper was brought in giving the names of a dozen leading German professors dismissed by the Nazi regime on racial or political grounds. He returned immediately to the London School of Economics, of which he was at that time director, to organize an effective answer to Hitler's attack on scholars. "On my way home from Vienna, I had my first intimation of what terror may mean to the individual when justice has become the will of a sadistic tyrant. There travelled with me a German professor slightly known to me, not one of those already listed for dismissal but one who might be on a new list. He was in a state of panic all the way because in the next compartment was a youth, little more than a boy, whom he took for a Nazi agent, detailed to keep watch on him and hand him to the police. My friend's fears may have been imaginary, but his panic was real."

By May of that same year the Academic Assistance Council had come into being, because of the spontaneous and emphatic reaction in British universities to Hitler's racial policy. John Maynard Keynes was among the first to contribute. Under the stimulation and guidance of Lord Beveridge, the Council became something of a specialized labor exchange, always putting displaced scholars into contact with universities. Originally conceived as a temporary body to deal with a temporary persecution, the Council in 1938 became the more permanent Society for Protection of Science and Learning, as its founders realized that persecution, in one form or another and in one country or another, was here to stay. Lord Beveridge, now president of the Society, has written the story in *A Defence of Free Learning* of British efforts in the last twenty-five years to alleviate that persecution. The book is less dramatic than its subject would indicate, being largely an organizational history of the Council and its successor, the Society. However, it comes as a relief to find a dispassionate account of an explosive topic. The vast majority of books dealing with Nazi racism, understandably but regrettably, give vent to an intense emotionalism which obscures, paradoxically, the true extent of the horror and tragedy involved. Lord Beveridge and the others who actively took up the Nazi challenge by defending "free learning"—actively, since, as he once remarked, "protests butter no parsnips"—performed a remarkable service, both to England and to the free world. His brief account is worth reading, if only to serve as a timely reminder of the relative values of western and totalitarian societies.

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LEO F. SOLT. *Saints in Arms: Puritanism and Democracy in Cromwell's Army* (Studies in History, Economics and Political Science, XVIII.) Pp. 150. Stanford, Calif.: Stanford University Press, 1959. $4.00.

This small book has a large and complex theme, the relationship of Puritan theology to democratic thought. For some time the students of English constitutional history have looked to the seventeenth century for the origins of democratic ideas, and especially to the religious radicals of Cromwell's army. This interpretation has rested on the analogical extension of equality in the church to equality in the state. Somewhat sceptical of this, Professor Solt has re-examined it in the light of the polemical tracts and sermons of the chaplains in Cromwell's New Model Army, whom he treats as the official educators of the troops in both religion and politics. He selects for his study especially the writings of William Dell, John Saltmarsh, and Hugh Peters.

The author subjects the polemical tracts and sermons of the chaplains to a careful logical analysis. His several chapters set forth the polarities of such religious and political ideas as liberty and authority, unity and uniformity, separatism and millenarianism, grace and nature. The key to understanding seventeenth-century political thought, the author thinks, is the argument whether government should be by "good men or good laws, whether by grace or nature" (p. 4). From the study of the chaplains' Antinomian theology, Professor Solt concludes that they believed "in government by the saints" (p. 69). He finds no suggestion of the social contract in their thought. Because the chaplains did not hold the doctrine of universal salvation, he concludes, they could not have believed in equality within the state; because they did not hold to a specific form of church government, they could not have developed a specific form of political government; because they held millenarian views, they therefore favored an authoritarian government by the saints (p. 72).

The analytical method of deducing political systems from theological premises has its difficulties. It is difficult, indeed, to deduce a system of thought from the sermons and tracts of men who were not systematic thinkers. The analysis of meaning hardly explains the power of the sermons which instilled a fighting spirit into Cromwell's soldiers. It may be just as useful to view Puritan theological ideas as social symbols which produce group and class solidarity, or as myths which defy analysis but which stir men's emotions and move them to act. Although Professor Solt's study is useful in pointing out the complex relationship of political and religious ideas, it will hardly put an end to the perennial debate over the origins of democracy and over the character of Puritanism.

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ARTHUR LAYTON FUNK *Charles de Gaulle: The Crucial Years, 1943–1944.* Pp. xv, 336. Norman: University of Oklahoma Press, 1959. $5.00.

The two event-filled years between the fall of France in June 1940 and the invasion of North Africa in November 1942 are recounted in this book as the setting for De Gaulle's emergence as a political figure. The "crucial years" 1943–44 are then analyzed in terms of their most significant events—successive chapters dealing with the Casablanca Conference, formation of the French Committee of National Liberation, its recognition by the Anglo-Americans, the Liberation of France, and the installation in Paris.

The account has merits. The prose is fluent; the story engages our interest. Even the plot is clear—no small accomplishment with so varied an array of characters and events to be brought into orderly sequence. The book is thus a useful addition to the conventional history of international relations. It will be read with pleasure and profit by all concerned with the critical role of France in the Allied coalition during World War II.

The other commitment taken by Mr. Funk, however, is less well met. He offers us international history as the matrix for biography. As biography, however, the book is sterile Once again we are shown De Gaulle as Churchill's "cross of Lorraine" and Roosevelt's "unwilling bride." But this familiar clash of public personalities is not illuminated by insights or data of any explanatory value. The goal of this book is, as I understand it, to explain De Gaulle's triumphant rise to French leadership despite the latent and manifest hostility of the Allied leaders. What in De Gaulle appealed so powerfully to Frenchmen while alienating others? What inspired popular trust in him among the peoples of the Allied coalition while generating deep distrust among their leaders?

For clarification these complex questions require investigation of the relationship between power and personality in contemporary political life Had Mr. Funk dealt successfully with these questions, he would surely have enlarged our understanding of the political malaise from which the West still suffers today. The route which led the Allied coalition from a conservative military strategy in World War II to a defensive political posture in the postwar years would have been clarified.

But Mr. Funk shows neither the range of interests nor the array of skills needed for this task. The concepts and techniques of inquiry which have brought individual personality and political behavior into a single research focus in recent years lie outside his ken. Accordingly, he is content to conclude his labors with the banal comment:

"Between 1940 and 1945, De Gaulle developed enormously from the unknown specialist in tank warfare to the statesman coping with international and domestic affairs; but in spite of a considerable and often unacknowledged diplomatic skill, he never acquired that magnanimity, that breadth of view, or that sense of humor which might have altered the tragic course of his career and rendered his achievement far greater than it was."

But the profound questions underlying this "tragic course"—a great achievement undermined by its own intrinsic limitations —are never confronted

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ALEXANDER L. GEORGE. *Propaganda Analysis: A Study of Inferences Made from Nazi Propaganda in World War II.* Pp. xxii, 287. Evanston, Ill.: Row, Peterson and Company, 1959. $6.00.

Several wartime agencies as well as the intelligence divisions of the Armed Services concerned themselves with foreign and especially enemy propaganda during World War II, but none did so as comprehensively and intensively as the Foreign Broadcast Intelligence Service (FBIS) of the Federal Communications Commission (FCC). To the Analysis Division it intrusted the task of examining all broadcasts and available newspapers for indications not only of the policies and purposes behind propaganda but also of the military and other intentions of the governments in question. The resulting inferences were circularized to the interested offices.

While the author has not undertaken the probably futile task of studying their influence upon the conduct of the war, Dr. George, a junior official in the Analysis Division and presently a member of the staff of the Rand Corporation, has checked the inferences relating to Nazi Germany with the voluminous but incomplete historical evidence in captured documents, memoirs, and interviews to determine their accuracy The difficulties incurred in this endeavor prevented a completely reliable result, but he believes that the analysts were successful in more than 80 percent of their inferences. This record of achievement constitutes, in his view, a sound basis for the formulation of a rational technique of propaganda analysis, itself in large part the product of inferences since the wartime analysts rarely explained the reasoning behind their conclusions. The case was first developed in reports to the Air Force whose funds financed this investigation, and the present study was accepted by the University of Chicago as a doctoral dissertation.

In its essentials, this highly rationalized technique of propaganda analysis, whose complexities are not always clarified for the layman by the author's abstruse terminology, largely centers on a detailed breakdown of "the logic of the situation," a concept which he accepts too readily as having the unreserved approval of historians. The final result is doubtless an advance over wartime procedures, but that it goes far towards transforming what he calls "intuitive art into science" is less certain. Unlike other intelligence methods, the reliability of its results can only be determined much later and in some instances only after the enemy's complete defeat as in the case of Germany.

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EARL R. BECK. *The Death of the Prussian Republic: A Study of Reich-Prussian Relations, 1932–1934.* (Florida State University Studies, 31.) Pp. x, 283. Tallahassee: Florida State University, 1959. $6.00.

In this most interesting volume, Earl R. Beck, of Florida State University, makes an extensive comparison of what he calls the two Prussias. The old Prussia, with its *Kadavergehorsam,* Uhlan, and Junkers, dominated the politics of the German nation from 1871 to 1919. This was the militaristic, autocratic, authoritarian Prussia, epitomized in the iron-and-blood Chancellor, Otto von Bismarck, and lauded by Hegel and the school of Prussian historiography.

But the second Prussia emerging in 1918, says Dr. Beck, was a republic, one of the eighteen states making up the Weimar Republic. The political leaders of this second Prussia—the Otto Brauns, the Heinrich Brünings, the Carl Severings, the Wilhelm Marxs, the Joseph Wirths, and many others, sought zealously to transform democracy from vision into reality. These men, says the author, typify a tradition far more worthy of the name of Prussia than did Frederick William I, Bismarck, and von Hindenburg. They made the new Prussian government stable and efficient; they successfully countered communism on the left and Nazism on the right; they were the bulwark of the Weimar Republic.

This new Prussia died on July 20, 1932, when Chancellor Franz von Papen, utilizing the famed Article 48 of the Weimar Constitution, the clause which gave emergency power to the President, deposed the legal government of Prussia and took over control as "Reich Commissioner." From this time the Prussian Republic slid to its doom. Hitler enveloped it in even tighter federal controls and eventually throttled it. What might have been a leading democratic European state, a state that might have played a most important role in European history, died.

The author concludes with this hope: "May a new generation of German historians, those who are now searching the past with open and critical eyes, seek more their ideal among the little men of the day than among those who towered over the time by ruthless will and autocratic actions. The old Prussia of 'blood and iron' is dead The Republic of Prussia still lives as an experience and a tradition from which a new republic and the new states that compose it may well draw strength and pride" (p. 220).

The author tells the story of the death of the Prussian Republic by unraveling the complicated strands of politics and legal actions of the period. Readers will be impressed by his painstaking documentation and by the care he has taken to substantiate his main thesis. Others will feel that he has underestimated the capacity and staying power of his first Prussia, which by no means was obliterated by Versailles. Still others will disagree with the author's conclusion that republican Prussia was "a bastion of democracy." Neither the liberal nor the democratic tradition, in the Western rationalistic rather than the Prussian sense, was given a chance in Prussia since 1848 and in Germany since 1871 Even today there are many who are wondering what will happen in West Germany once the "strong, sure, authoritarian-democratic hand" of Adenauer is gone.

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JOACHIM REMAK. *Sarajevo: The Story of a Political Murder.* Pp. xv, 301. New York: Criterion Books, 1959. $5.00.

Forty-five years after the assassination of the Austrian Archduke and his wife the actual planners and their motives remain uncertain. For almost five decades suspicions have centered at one time or another upon the youthful assassins, Russia, Serbia, Austrian enemies of the Archduke, and countless other suspects, many simply products of hyperactive imaginations. This book, the latest recreation of the Sarajevo events, advances yet another point of view in an absorbing and adroitly written narrative that frequently takes on the air of the best in suspense fiction.

A history teacher at Lewis and Clark College in Oregon, Dr. Joachim Remak's thesis is that the entire conspiracy was planned and directed by Serbian Army Intelligence chief, Colonel Dragutin Dimitrijević, known also as "Apis," and his Black Hand society. Nikola Pašić, Serbia's prime minister, was aware of the plot and ultimately sent an "inadequate warning . . . neither formal nor specific" to Vienna's finance minister who promptly belittled and ignored it. Serbia, Dr. Remak argues, certainly believed it could benefit from the conspiracy and their politicians used the Black Hand whenever convenient or necessary to advance grandiose dreams of a Greater Serbia. As it happened, in 1914 Serbia's aims coincided with the ultranationalistic Black Hand, namely to destroy the Heir Presumptive because of his support of Trialism—transforming the Dual Monarchy into a Triple Monarchy, with the Empire's Serbs receiving autonomy—and the danger that presented to Serbian expansionists.

How valid are these conclusions? In the light of Dr. Remak's own reservations for in one footnote he concedes ". . . having considered all the fragmentary evidence that is available, it seemed plausible. . . . What cannot be claimed for it is that it is based on incontrovertible fact" and considering that no concrete primary evidence linking Apis and the Black Hand with the murders has ever been offered, it is difficult to accept this well-told tale as absolute truth. Since neither Apis nor the Black Hand were ever mentioned at the trial of the assassins, Apis' dubious final testament alluding to Sarajevo and implicating no one is hardly suitable supporting evidence. Moreover, it should be emphasized that by 1914, the Archduke had abandoned Trialism as unworkable. What, then, becomes of the motive?

One day Serbia's still closed diplomatic archives may reveal what really happened. Until then, while the definitive apportionment of responsibility will have to wait, it appears that a more accurate version would have Princip and his friends, aroused by Pan-Serbian nationalism, planning and committing the murders themselves with the encouragement but not the outright co-operation of Avis, the Black Hand, and the Serbian government.

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GIORGIO LOLLI and others. *Alcohol in Italian Culture: Food and Wine in Relation to Sobriety among Italians and Italian Americans.* (Monographs of the Yale Center of Alcohol Studies, No 3.) Pp. xv, 140. Glencoe, Ill.: Free Press with the Publications Division, Yale Center of Alcohol Studies, 1959. $4.00.

This excellent monograph is the third in a series: the first dealt with the Jews, a group distinctive for the absence of alcoholism, and the second with the "skid-row" type of chronic alcoholic The present volume deals with Italian Americans in New Haven, Italians in Rome, and a stratified sample of Italy's entire population. The study concentrates on the place of liquids, particularly milk and alcoholic beverages, in the informants' diets from infancy through adulthood It is made abundantly clear that the purposes, values, and social contexts of drinking in the Italian and Italian American cultures are markedly different from those of American society at large.

The discussion compounds sociological, psychiatric, and nutritional theory and data to show: (1) breast feeding is supported by Italian culture, being an almost universal practice considered both good and natural, (2) breast feeding minimizes the risk of an undifferentiated craving in adulthood for this physiological and psychological satisfaction which the alcoholic in American culture missed—or thinks he missed—in infancy, (3) alcoholic beverages, particularly wines, are food for Italians, drunk mainly at meals or as a fortifying thirst-quencher during manual labor, (4) the drinking of alcoholic beverages on a full stomach, especially with the slowly absorbed carbohydrates of Italian cuisine, reduces the effects of alcohol on the central nervous system, (5) drinking among Italians does not have the purpose of inducing sociability or providing escape, (6) Italians generally drink alcoholic beverages in the bosom of the family, (7) drinking alcoholic beverages is supported by Italian culture, being an almost universal practice from childhood, and it is considered both beneficial and normal.

The authors' interpretative scheme leads to one important question. What is the dominant factor in Italian sobriety or the principal cause of widespread alcoholism in many other cultures? It would seem that breast feeding and familism, the two most striking features of Italian culture in the authors' analysis, are insufficient even though necessary conditions for sobriety. It is the reviewer's impression that several familistic cultures where breast feeding is general, such as rural Ireland and Poland, have a high incidence of inebriation. It is hoped that further cross-cultural comparisons in this series will seek to show specifically what it is in Italian nursing practices and family life which reduces alcoholism. However, the monograph admirably fulfills its purpose by narrowing the field of inquiry to this area, thus pointing the way for additive research which promises to be of great theoretical and practical importance.

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CYRIL EUGENE SMITH. *The University of Toulouse in the Middle Ages: Its Origins and Growth to 1500 A.D.* Pp. vii, 244. Milwaukee, Wis.: Marquette University Press, 1958. $7.00.

Professor Smith, in order to explain "obvious lacunae" in the present volume, which is devoted to a narrative of "the origin and external history of the university," states that he hopes to publish another volume on the constitutional and administrative history of the university. The wisdom of this organization may be questioned, for in the present volume the failure to discuss the structure and function of the university is felt as a very obvious gap. The author touches on many subjects: the early history of Toulouse and previous educational activity in the region; the circumstances and personalities connected with the foundation of the university in 1229 to combat heresy, possibly the third instance of deliberate foundation rather than gradual growth in the Middle Ages; initial difficulties in a hostile land; the growth in personnel and privileges; relationships with monastic houses; the foundation of colleges, particularly numerous at Toulouse; the close ties with the Avignon papacy; conflicts with civic officials of Toulouse; conflicts with royal officials and relations with the monarchy; and the increasing litigation in and control by the *parlement* of Toulouse in the later fifteenth century.

Unfortunately, the organization by chronological narrative and the focus on external influences prevent the author from giving a coherent picture of the core of his study, the university itself. Institutions are elusive and complex historical subjects whose structure and function need careful description and analysis before the relevance and significance of many facts about the institution can become clear. The disadvantages of a chronological treatment are very apparent in the chapter on "The University and the *Parlement* of Toulouse, 1434–1500," where, interspersed among descriptions of relations with the *parlement* and paraphrases of litigation on diverse subjects, the author discusses royal visits to Toulouse, the requiem mass for Charles VII, university participation in the Council of Basel, the financial situation of the university, fire and plague, a disputed archiepiscopal election at Toulouse, the appearance of printing, abuse of letters of scholarity, the foundation of a college, and financial affairs of other colleges.

Although the book is little concerned with medieval education as such, does not focus on any central problem, and has no unifying thesis, it is rich in details about personalities connected with the university, sedulous in correcting mistakes in dating of other historians, and valuable for its description of many incidents in the university's history. It is unlikely to be of interest to general readers wishing to understand the nature of a medieval university, but as a meticulous chronicle it will be useful for specialists.

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H. H. SCULLARD. *From the Gracchi to Nero: A History of Rome from 133 B.C. to A.D 68.* Pp. xi, 450. New York: Frederick A. Praeger, 1959. $6.00.

As the title indicates, this work by the well-known author of *A History of the Roman World from 753 to 146 B.C.* and *Scipio Africanus, Roman Politics: 220–150 B.C.*, covers a most critical period in Roman history in which the collapse of the Republic and the formation of the Principate or Empire took place. This is one of those historical epochs which evokes perennial interest, and consequently a fresh treatment of it is always welcome, particularly at this time, for many specialized studies written in recent years and some newly discovered source materials require evaluation or interpretation.

The author does not advance any one special cause for the breakdown of the Roman system of republican government. Rather, he wisely lets the sequence of events from the Gracchan attempts at reform to Caesar's victory in the civil wars speak for itself. As a result, we see that the responsibility rests upon the human factors involved, not upon economic or social forces. The "fall" of the Republic was not inevitable; it was the result of conflicting class and individual interests which could have been resolved, at least in their early stages, by statesmanlike and patriotic courses of action taken at various crises by the parties involved. As it turned out, however, the major share of blame falls upon the dominant faction in the senatorial oligarchy which adopted an attitude of no compromise whenever their traditional position of power and privilege seemed threatened by proposals for reasonable and necessary reforms. But their opponents cannot be held guiltless, especially when their personal ambitions led them into actions necessitating the overthrow of constitutional authority. After Caesar, some form of monarchy was inevitable, and it is to the credit of Augustus that he created one which, in spite of its potential weaknesses, revealed already under his immediate successors, served the needs of the Empire so long and so well.

Anyone reading this book will be struck by the author's realistic and judicious appraisal of the leading characters of the period. The Gracchi, Marius, Sulla, Pompey, Caesar, Antony, Augustus, and the Julio-Claudian emperors appear as intelligible human beings neither wholly evil nor wholly above criticism. Scholarly as it is, this work can be read with appreciation by all those interested in historical narratives and interpretations. Students of Roman history in particular will appreciate the Notes which offer useful discussions of the sources for each chapter and also select Bibliographies. There is a good Index and some useful maps.

A. E. R. BOAK
Professor of History, Emeritus
University of Michigan

ASIA AND AFRICA

DAVID S. NIVISON and ARTHUR F. WRIGHT (Eds.). *Confucianism in Action.* Pp. xiv, 390. Stanford, Calif.: Stanford University Press, 1959. $8 50.

Chinese and Japanese studies in the United States and Great Britain have come of age. A generation or so ago there were good monographs on the founders of Chinese thought in the classical period and on some of the principal spokesmen from the first to the seventeenth centuries, but there was little depth and no intimate picture of the chief institutions of government or of social life. Now, due to the awakened interest in our universities and to the support of foundations since World War II, there are a number of scholars, well trained in the languages and in their disciplines, working on neglected areas of eastern history, social, economic, intellectual, and other aspects.

Here we have a fine analysis of the rules of Chinese clans based on a long series of genealogical records, which shows how the elders of the clans tried to force their members to behave. Here too is a study of a well-kept record of a charitable estate, founded in 1050 and continued to 1760, revealing the efforts of the founder and his successors to benefit the less fortunate members of his clan by establishing ancestral halls, charitable schools, and income-producing estates as well as a perspicacious study of the Chinese censorial system in government which did so much through remonstrance and surveillance of the entire bureaucratic machinery to preserve the best in the state—a system which grew out of both Confucian and legalist streams of thought. It is noteworthy that even the Mongol conqueror Kublai Khan who reigned from 1260 to 1294 was so imbued with the need of this arm of the government that he remarked: "The duties of the Censorate's officials lie in speaking out straightforwardly. If we should by chance commit improprieties, let them speak out vigorously, without concealment and without fearing others."

The classics of the Confucian school were long known to the Japanese and studied at Nara and elsewhere for a thousand years before Confucianism became in the 17th century "the strongest intellectual and ethical force in Japan." The teachers of this school of thought and their part in Japan prior to the restoration of imperial rule are given their rightful due in one essay, followed by an intimate picture of the tutor and chief advisor to the Meiji emperor from 1871 through 1890, a man who began his work when he was fifty-three and the young monarch eighteen, and who saw him nearly every day of those twenty years. So did Confucianism exert its power during the period when the influences of the west were nearly at their peak.

These remarks touch on only five of the dozen papers published herein, all of which are worth reading for their penetrating and and realistic appraisal of a major way of life and thought in eastern Asia.

L. Carrington Goodrich
Dean Lung Professor of Chinese
Columbia University

R P. Dore. *Land Reform in Japan* Pp. xvii, 510 New York: Oxford University Press under the auspices of the Royal Institute of International Affairs, 1959. $8.80.

At last we have a comprehensive, objective, well-documented, and interesting evaluation of the much-touted land reform brought about under the Allied occupation in Japan. Tenant-cultivated rice land was reduced from over 50 per cent to only 10 per cent, and now only 5 per cent of farm families rent more than 90 per cent of their land No landlord may retain more than 1 *cho* (2.45 acres) of tenanted land Thus, land is hardly an issue in Japanese politics any longer That is, tenant-farmer movements of the prewar type, which are well treated in the book, find no strong raison d'etre which is a loss to the Left. It is rather the small and ex-landlords who find sustenance for their discontent in the residual tenancy, and this is "the greatest single cause of dissension in Japanese villages today." The land reform was also incomplete in that it did little to do away with fragmentation of the land. Nevertheless, the conservative politicians have accepted the reform and give the discontents little encouragement, for the mass of votes come from the owner-cultivators who were formerly tenants.

Actually the book goes well beyond a narrow discussion of the land reform as such. It dissects the whole problem of tenancy in the development of Japanese agriculture and develops the implications of the land reform for the farm economy, the village social structure, and national politics today. By relieving the cultivators of the rent burden, by giving them responsibility for improving their own land, and by making the small and medium owner-cultivator more important politically, land reform can be counted high among the factors that have raised the standard of living of farmers in postwar Japan. Also, by reducing class divisions, land reform has paradoxically strengthened village solidarity, something usually denounced as "feudal" but which the author shows can also lead to progress. Finally, the solidarity of the agricultural population, encouraged by the land reforms, has caused a congruence between conservative and socialist agricultural policies. Both appeal to the village as a whole. The socialists are trying to outbid the conservatives; they apparently seek in vain for a new departure.

The author, a sociologist and associate professor of Asian studies at the University of British Columbia, has a fine command of Japanese. He was thus able to detect and record the nuances of socio-political attitudes of respondents in the extensive conversations and interviews he conducted as well as make full use of Japanese research, sources, and statistics.

In the opinion of this reviewer, *Land Reform in Japan* will take its place among the classics in the field of studies of modern Japan. In addition, it will undoubtedly prove of great comparative importance for all who are interested in the "land question" in any industrializing society.

GEORGE O. TOTTEN
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Boston University

GEORGE ALEXANDER LENSEN. *The Russian Push Toward Japan: Russo-Japanese Relations, 1697–1875.* Pp. xv, 553. Princeton, N. J.: Princeton University Press, 1959. $10.00.

This very large and solid volume by Professor Lensen of Florida State University is likely to prove the definitive work in English on Russo-Japanese relations between the earliest contacts and the border settlement of 1875, which assigned the whole of Sakhalin to Russia and all the Kuril Islands to Japan. It seems improbable that enough further material will be uncovered to induce other scholars to retrace in detail the whole of this story he has covered so fully and well.

As the title suggests, the subject is presented mostly from the Russian point of view rather than the Japanese. The author has described in some detail the Russian background of the relations with Japan and has ably exploited the accounts of the Russian naval officers, explorers, adventurers, and diplomats whose acts form the main line of the narrative. He has also made wide use of Japanese and other sources, though usually as supplementary materials to the Russian. Those interested primarily in Japan will wish that he had given as much attention to the Japanese background as the Russian and had made a greater attempt to present the story from the Japanese point of view. As a consequence, the first two chapters of Professor John A. Harrison's *Japan's Northern Frontiers* (University of Florida Press, 1953) will still be useful as a look at this subject from the Japanese point of view, though Harrison's much briefer book is devoted primarily to the Japanese development of Hokkaido rather than to Russo-Japanese relations.

To call a book "definitive" may make it sound ponderous and dull. This is not true of Professor Lensen's volume. For all its length and detail, it makes fascinating reading. Such accounts as the flight of the prisoner Benyovszky from Kamchatka by way of Japan to the West, the mission of young Lieutenant Laxman, the imprisonment of Golovnin, the negotiations of Rikord and Takadaya, and the missionary work of Bishop Nikolai all make excellent stories, wholly aside from their broader historical significance. The author has told these stories well, letting the original records speak for themselves where possible. His balance and historical judgment seem sound, his writing is competent, and the volume is handsomely published with good maps and illustrations. One can only look forward with great expectation to Professor Lensen's promised second volume on Russo-Japanese relations since 1875.

EDWIN O. REISCHAUER
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Harvard University

GEORGE MCTURNAN KAHIN (Ed.). *Governments and Politics of Southeast Asia.* Pp. xvii, 531. Ithaca, N. Y.: Cornell University Press, 1959. $6.50.

This is a most welcome book for at last we have a single volume that provides a reliable introduction to the countries of Southeast Asia, and that can be strongly recommended to the student, the general reader, and all who have a professional interest in the region. Professor Kahin has worked out with his contributors a common framework within which they are able to elaborate the distinctive features of each country and also point up the shared characteristics. The separate contributors follow a common outline in treating the six major countries of the region. Each country study begins with a brief historical résumé covering the pre-European period as well as colonial and postcolonial developments. With the advantage of this long-range perspective each author turns next to the contemporary scene and describes the outstanding social and economic characteristics of his country. The sections on the political process are divided between descriptions of the formal institutions of government and the informal patterns of behavior. The final sections are devoted to the major problems confronting each of the countries.

Southeast Asia is in such a state of flux that one cannot but feel sympathy for the authors and their problems of choice. They have sought to give a clear and precise picture of the ways in which the various governments of the region operate, and yet they are also aware of the fragile and ephemeral nature of most formal institutions in these developing societies. This situation calls for a judicious balance between general analysis and description of the current scene. The contributors have attempted to give more complete reports on the formal arrangements of government than are common in books on Asia, and in doing so they risk attributing excessive permanence to what are probably only temporary phases of institutional change. In the main, however, they have resisted the temptation of being unduly concerned with most recent developments. Possibly they have been less successful in weeding out accounts of developments which at an earlier time seemed significant, but which have not had lasting consequences. Here again is the problem of finding the appropriate perspective for viewing an erratically changing world.

The composite picture of Southeast Asia that emerges from the country studies is one of diversity and confusion on the surface and a few all-pervasive fundamental problems. Throughout the region there is a desperate need for economic growth, but such development is inhibited by a lack of political unity. Governments can dominate the scene, for other groups are all so weak; but they lack the capacity to control and direct events In spite of periods of high hope the basic theme of Southeast Asia is still that of a vicious circle of economic stagnation and political disunity. In countless ways this volume points to all the problems that must be overcome if the modernizing leaders of Southeast Asia are to realize their aspirations.

Although Professor Kahin is not the author of any of the chapters, his influence is to be noted throughout the book, and not just because of his editing skills and his respect for clean, precise prose. Even more, this book is a testament to the remarkable role that George McTurnan Kahin has played in training a generation of American scholars of Southeast Asia. This book is built out of the relationship between teacher and student, and it is rare for each to do so well by the other.

LUCIAN W. PYE
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OLAF CAROE. *The Pathans: 550 B C.–A.D. 1957.* Pp. xxii, 521. New York: St. Martins Press, 1959. $12.50.

Sir Olaf Caroe was right in superimposing a portrait of a Pathan militiaman upon one of Alexander the Great on the cover of his book, just as it is right that most Western visitors should first meet the Pathans at Peshawar airport looking out toward the gaunt hills of the Khyber Pass, as the Pathan history and geography have played a very great part in molding their character. But Caroe was also right to devote a long, though difficult, first chapter to the semimythological genealogies of the Pathan tribes and subtribes. These genealogies form the basis of the folklore of the Pathans, and it is only through some understanding of these genealogies and this folklore as well as of the history and geography that one can hope to understand the Pathans and their attitude to the modern world. These attitudes are so different from those of the West that among the Westerners who have met them there must be very few who do not have strong feelings either for or against the Pathans It seems likely that even the misguided few who detest them will find Sir Olaf's account illuminating and fascinating; and what is more to the point, it should help them to mend their misguided views.

But it should not be only those who have or will have dealings with the Pathans themselves who will find this book useful The Pathan homeland stretches across the mainland communications between India, the Middle East, and Central Asia. In telling the history of the Pathans, Sir Olaf necessarily sees the complicated history of all three regions from a Pathan viewpoint which though sometimes difficult should be illuminating for those concerned with any one of them. It is useful to be reminded that the Pathan contact with Europe stretches back at least as far as the Greeks It is startling to realize that in the judgment of many of them the contact with Alexander is still of more importance than their more recent contact with the British, but by the end of this book the reader begins to understand this judgment.

It is extremely difficult for anyone to understand and explain the ideals and values of another people. Sir Olaf has succeeded admirably. One only wishes that similar books were available on all the many peoples of Asia. In the meantime anyone interested in the complexities and differences between the different cultures of South Asia would do well to read *The Pathans.*

T. F. WISE

Dominion Bureau of Statistics
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SAUL ROSE. *Socialism in Southern Asia.* Pp. 278. New York: Oxford University Press under the auspices of the Royal Institute of International Affairs, 1959. $4.50.

The importance of the author's subject lies, in part, in his conclusion that socialism is the "predominant ideology" in Southern Asia and that there are at least six national governments that describe themselves as "Socialist," these countries embracing the majority population of the region. Beyond this, his subject gains added significance from the fact that some form of democratic socialism is the only real alternative to communism or indigenous totalitarianism in South Asia, as the author implies but does not, unfortunately, develop in a comparative manner. By South Asia, he means South and Southeast Asia, from Pakistan to Indonesia, an area, he feels, in which socialist movements have "certain common features" not shared by others, such as Japan or Israel Examples of such shared characteristics are the newly won independence of those nations, with the exception of Thailand and Nepal, an embryonic development in the womb of nationalist movements which is responsible for a special type of affinity between socialism and nationalism peculiar to South Asia, and common problems of economic development and national cohesion.

A central problem is to differentiate between competing socialisms in each country where all or most parties, those in and out of power, claim to be socialist. This is a problem not only for the scholar attempting to understand the politics of these countries but also from the practical standpoint, for the Asian Socialist Conference, which, as the self-constituted organization of Asian socialism, has the onerous task of selecting the true Socialist party in each country and rejecting the others. This difficulty also plagues the various national electorates who are asked to choose, as in India, for example, among the socialism of Nehru's Congress party, the Praja Socialist party, and the Communist party of India.

The author treats socialist parties descriptively on a country-by-country, brief survey basis, sandwiching them between valuable chapters on the first and second Asian Socialist Conferences held in 1953 and 1956, which have sometimes appeared as a potential rival to the established Socialist International based in Europe.

One must take issue with his assertion that differences between Asian and Western socialism can be reduced to mere questions of "tempo," over the rate of colonial liberation and industrialization and that the former is gradually moving in the direction of Western socialism. This conclusion must seem doubtful, even to the author, when, in a latter part of the book, he indicates the primary importance of the charismatic leader, points to the acute problem of the peasantry, and reveals recent Asian socialist doubts about the all-important role of the state as a means and a newly found appreciation of the need of decentralization and local autonomy.

Nevertheless, this is a valuable book for understanding of Asian and socialist politics.

Thomas A. Rusch
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Sydney Nettleton Fisher. *The Middle East: A History.* Pp. xiv, 650, xxxi. New York: Alfred A. Knopf, 1959. $8.95, trade edition; $6.75, text edition.

He who reviews a history of the Middle East is confronted with some of the difficulties of its author. The perplexities of this reviewer are augmented by reading Elizabeth Hardwick's "The Decline of Book Reviewing" in the October Harper's Magazine with her comment about reviews written by one ". . . who had written a book like it, or about the same country or the same period."

So complex are the problems of a historian who undertakes to write a one-volume history of the Middle East, that only carping critics would expect more than a competently written book useful for those who wish to increase their knowledge of the area. Such a book Professor Fisher has written. He has accomplished well his stated purpose of providing ". . . a brief account of the contemporary Middle East scene so that the beginning college student or general reader" can obtain a historical background for an understanding of its present-day problems.

Over half of the book is devoted to presenting such a background. The first chapter deals inadequately with the physical features of the area, the second briefly with pre-Islamic history. Byzantine Christian heritage is given little attention in both chapter two and chapter eleven. In striking contrast Fisher writes in considerable detail of Mohammed, the Koran, the spread of Islam and Moslem theology, and law and culture under Arab leadership.

The author's special interest is in the Turks whose history he traces in eight chapters from the advent of the Seljuks to the end of the eighteenth century. He vividly describes the rise, the achievements, and the stagnation and subsequent decline of the Ottomans.

Part III entitled European Imperialism is not an analysis of the nature of European imperialism as it functioned throughout the Middle East and of its complex and multiform impact upon internal development in the Ottoman Empire. It is no easy task to weave into a meaningful pattern all the threads of Middle Eastern history from the reforms of Sultan Selim III and Napoleon's invasions of Egypt in the nineteenth century to the Young Turk Revolution and the rise of Arab nationalism.

The latter part of the book falls into two divisions: the first dealing with the period of World War I and its aftermath, and the second with World War II and developments up to 1958. Fourteen chapters are so crowded with factual data that there is little space for reading between the lines. The flood of events from 1914, which inundated and obliterated many of the old land marks and fertilized the Middle East, are treated in a way which tends to overwhelm the reader.

William Yale
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Boston University

Walter Z. Laqueur. *The Soviet Union and the Middle East.* (Praeger Publications in Russian History and World Communism, No. 81.) Pp. x, 366. New York: Frederick A. Praeger, 1959. $6.00.

In this volume, Mr. Laqueur, editor of *Soviet Survey* (London), and author of a number of works on the Middle East, traces the development of Soviet attitudes, policies, and interests in the Middle East from the period of the Bolshevik Revolution in 1917 to the present. Written under a research fellowship from the Harvard Russian Research Center and the Center for Middle Eastern Studies, the work is a welcome addition to our information and knowledge concerning that troubled area.

Essentially, Mr. Laqueur's volume is

divided into two parts, the first of which treats of the development of "the Soviet image of the Middle East as it grew and changed since 1917." The second is devoted to the "great break through," especially after the Soviet-Egyptian arms arrangement of September 1955, and discusses in some detail the development of policies during the critical and decisive years between 1954 and 1959. In the latter section the author discusses not only the arms arrangement, but the Suez conflict of 1956, the Syrian crisis of 1957, Soviet trade and aid in the Middle East, Soviet cultural policy, the development of Middle Eastern communism, and the evolution of Middle Eastern nationalism. In the end, the author believes there will be an inevitable conflict between Arab nationalism and communism. Three Appendices conclude the volume, dealing with (1) China and the Middle East, (2) Israel in the Soviet Mirror, (3) Iraq, Nasser, and the communists.

While the volume has perspective and balance, and is well written, the author might have provided his readers with a brief background of the Russian past in the Middle East, as necessary to any basic understanding of Soviet imperialism in that area. Secondly, there could have been a more ample treatment of Soviet policy and interest in connection with such specific Middle Eastern problems as those of Turkey and the Straits, Iran and oil, Palestine and Israel While Mr. Laqueur has evidently used Soviet sources extensively, aside from his annotations, he provides no extended Bibliography and cites few of the detailed western studies which have been made of various aspects of Soviet policy in the Middle East—studies which would guide the student beyond the confines of this relatively brief volume Mr. Laqueur's volume, however, represents only a beginning. It is now necessary to plunge more deeply into a most important and fascinating subject.

HARRY N. HOWARD

Beirut, Lebanon

ROM LANDAU. *Islam and the Arabs.* Pp. 298. New York: The Macmillan Company, 1959. $4.95.

If an objectively minded American wants to know "What makes an Arab tick?" he can find a dispassionate answer in Rom Landau's book, *Islam and the Arabs.*

Current developments in the Arab World are understood primarily by the so-called "experts" in that area or by emotional national partisans. Mr. Landau's book makes it possible for the average American citizen to understand more thoroughly why issues and conflicts arise in the Near East and in North Africa.

Most people in the United States rely unwittingly upon "spot news" in their daily newspapers for their basic knowledge of international affairs—a rather tragic dependence upon a kind of reportorial coverage that overemphasizes assassinations, coup d'etats, and violent upheavals. Too often "spot news" fails to provide a balanced interpretation concerning impacts that directly or indirectly brought about the current crises. Obviously, what has happened yesteryear influences what is happening today, and Mr. Landau's book provides the necessary background so essential in any fair-minded attempt to understand a foreign people whose culture and religion contrast sharply with those in the West.

In this concise and well-arranged history of the Arab peoples, the author emphasizes the extremely important role that the Islamic religion has played, and is still playing, in the social, economic, and political life of the Arab. Americans who sincerely want good will between the Arab world and the United States cannot impartially evaluate the political events now occurring in the Near East and in North Africa without first realizing what Islam means to the Arab. Mr. Landau's book fills such a need.

In a broad sense *Islam and the Arabs* can serve as a well-tempered study for anyone who goes to the Arab World in the diplomatic service, as an employee of an American corporation or simply as a tourist. In its thirteen chapters the book outlines the developments that led Mohammed to found a new monotheistic faith, the expansion of the Muslim empire under several dynasties, the impact of the Crusades, and the effect of Islamic institutions on Europe through the Muslim conquest of Spain.

The significant role that Islam played in bringing philosophy, astronomy, mathematics, geography, medicine, and chemistry to Europe then living in darkness is neatly documented in succeeding chapters. Each chapter concludes with a detailed chronology that links the Muslim World with developments contemporary to events in the West. This is an excellent reference.

In the final chapter, "Problems of the Present Arab World," the author explains why Arabs in the present decade are "strongly anti-colonialist and therefore almost automatically anti-West" (p. 239). He also explains why the Arabs feel they were betrayed "by the West after World War I" and how this betrayal led to "the subsequent deterioration in the relations between the Arab World and the West" (p. 240).

From his long experience in Muslim-Arab affairs, Mr. Landau gives this piercing summary that every Westerner should remember whenever political crises in the Arab World seem to perplex him:

"To understand the trend of modern Arab thinking and Arab policies we must try to view the relevant problems not from our Western point of view, but from that of the Arabs themselves. For it is ultimately their point of view and not ours—however erroneous and prejudiced the former may appear to us to be—that decides their attitude towards the world and their decision to prefer one set of ideas to another" (p. 235).

JAMES BATAL

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American University in Beirut

WILLIAM R. BASCOM and MELVILLE J. HERSKOVITS (Eds.). *Continuity and Change in African Culture*. Pp. ix, 309. Chicago: University of Chicago Press, 1959. $7.00.

The collection of papers in this volume, all prepared by professional anthropologists, is noteworthy for its demonstration of the magnitude and nature of the problem of cultural change in Africa, south of the Sahara

One of the most momentous developments of our decade is the rapidity with which political change is taking place in Africa. Less than ten years ago, besides the Union of South Africa, three independent states existed in Africa—Egypt, Liberia, and Ethiopia. Since then Libya, Morocco, Tunisia, Guinea, and Ghana have become independent, while the former French West Africa and French Equatorial Africa have achieved a degree of autonomy in the French Community Within the next year, Nigeria and Somalia will become independent, and there are indications that some of the members of the French Community and other African territories will shortly move towards complete independence. The independence snowball is rolling rapidly in Africa, and its momentum apparently will not be stayed until political independence and autonomy is attained by most of its people.

These climactic political changes are part of the process of transition from traditional and primitive society to modernism which is taking place in Africa under the impact of western civilization. It is no secret that the attainment of independence and self-government does not mean that the new African states have completed the transformation to modern society. Political independence, particularly when achieved rapidly as is the case in many of the African territories, hardly changes the people and their pattern of life overnight. It must be borne in mind that the changes resulting from western influences in Africa take place not only in the political realm, but also in other areas, such as marriage and the family, language, art, music, mode of life, and so forth. Not all of these changes are taking place at the some pace. Furthermore, traditional cultural patterns persist and affect the new political entities.

If we are to understand the peoples of Africa and the problems they face and create, particularly in their adjustment to modern political life, it may be stated axiomatically that we must pay close attention to the problem of cultural change in all its aspects. In this connection, the volume, *Continuity and Change in African Culture,* can serve a most useful purpose. Anthropologists and ethnologists will find much of interest in this volume from the viewpoint of their discipline and its methodology. But over and above this, other social scientists and laymen seeking an understanding of what is happening in Africa by going behind the headlines will find this volume of inestimable value. For example, the title focuses attention on what is changing and what is not changing; where change is taking place and where it is not. Not all of Africa is changing at the same rate, nor is all of Africa accommodating readily to change. Some societies are resisting it. One of the main themes of the book, often overlooked by students, is that the current African scene is marked not only by change but also by cultural continuity and that the emerging African states synthesize western culture and traditional African patterns.

BENJAMIN RIVLIN

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Brooklyn College

LETTERS TO THE EDITOR

September 30, 1959

Dear Sir:

I have read with some astonishment the review of my book, *The Spartacist Uprising,* by Professor Ralph Haswell Lutz. I am perfectly aware that neither my approach to the subject nor the conclusions reached will find the approval of everyone. However, I fail to see how any critical and open-minded reader of this book can come to the conclusion that it supports "the communist viewpoint of the German revolutionary movement of 1918–19" and "the line of polemics of the Communist parties." For example, Chapter 6 of the book entitled "The Revolution in Retrospect" contains a careful analysis of the communist interpretation of the German revolution and takes a very strong stand against the purposeful distortion made by the communist misinterpretation of historical facts. It is one of the author's main findings that the communist assertion that the showdown of forces between the left-wing radicals and those who wished to consolidate the political gains of the November revolution was not the result of deliberate actions by the Social Democratic Provisional government but "was the out-growth of the tension which existed between the highly antagonistic camps within the socialist movement . . ." (p. 214).

In his review, Professor Lutz employs the rather unfortunate method of attempting to discredit a factual statement by claiming that the same statement was also made by communists. This is hardly an academic approach. On the strength of it anyone can be discredited who happens to cite statements of fact which are also recognized as such by the communist side. How can Professor Lutz, an experienced and respected historian, disparage the findings of the author by stating that he had used readily identifiable communist source materials? How else would Professor Lutz want one to characterize the communist position and buttress, as the author has done, an effective critical view?

Professor Robert G. L. Waite, in a review to be published by the *Journal of Modern History,* has no difficulties in understanding the author's position which is neither in support of the communist point of view that the Social Democratic Provisional government deliberately provoked a showdown in order to destroy physically the left-radicals nor the "right-wing" version which places all the blame for the events on a premeditated communist attempt to seize power at that time.

Had Professor Lutz read the book more carefully he might also have appreciated the reasons which led to the inclusion of the introductory chapters preceding *The Spartacist Uprising* proper. They serve to underpin the author's thesis that the January uprising was the upshot of tensions which had been in the making for a considerable period of time.

ERIC WALDMAN

Marquette University

October 26, 1959

Dear Sir:

I have seen Professor Waldman's letter and do not find in it any reason for changing my original statements and conclusions. My review was based upon a careful reading of the text including "The Revolution in Retrospect," "Conclusions" and "Summary," which "modify points of view which are presented in the main chapters." The author commented on only two of the criticisms listed in the review.

The communist "view point of the revolutionary movement" was generally established by Lenin, Trotsky, Stalin, Radek, and a number of Soviet experts and social scientists who are partly listed in the scholarly, annotated Bibliography of T. I. Rainov (*Istorik Marksist,* 1933, Nos. 5 and 6), which includes all viewpoints from monarchists to anarchists. Clemenceau, Lloyd George, Wilson, Hoover and a galaxy of experts and scholars at Versailles presented western viewpoints of this revolution, but none agreed with Lenin and his lieutenants. My considered opinion is that Professor Waldman's evaluations resemble generally the communist viewpoint until he modifies them in the last twenty four pages of his monograph

Furthermore, "the line of polemics of the Communist parties" was fairly well drawn when the first so-called "anarchist Soviet Republic" was formed April 6, 1919, in Munich, which was then followed by the proclamation of a Communist state on direct instructions from Moscow.

The polemics of western statesmen, generals, and social scientists against the Socialist party were well known before the delivery of the Allied mantle note The author's polemic again resembles that of the communist parties rather than those of the West. Professor Waldman should stress the difficulties of the majority Socialists, the historic ministerial mistakes and decisions of the Commissars, as well as Ebert's stubborn resistance to Allied demands and Reuters' propaganda.

I deny that in my review or elsewhere I attempt to discredit a factual statement by claiming that the same statement is also made by Communists. I have cited in documentary histories and narratives numbers of communist sources. The author's first citation to the admitted communist propaganda publication, *Illustrierte Geschichte* (p. 10), has nothing whatever to do with communism but is his authority for socialist and trade union membership figures. Why cite communist propaganda when the best documentary sources are readily available? Professor Waldman has good reasons to quote from Lenin, Liebknecht, Luxemburg, Rosenberg, and other communist writers. To that I did not object.

The author states that *"one of his main findings"* is that the showdown of forces between the communists and the socialists was not as the communists assert, the result of deliberate action by the government (p. 214). But what communist social scientist makes the preposterous charge that it was? Moreover, who are the American economists and historians who support this "widespread assumption"? Liebknecht had accepted money, guns, and an alliance from Lenin, and with Pieck voted in the revolutionary committee for the overthrow of Ebert's government. Had the uprising succeeded, the armies of the victors would have occupied Potsdam-Berlin and the heartland of the Junkers. Professor Waldman should know and state forthrightly that the Allies would never have made peace with a Berlin Communist government which in January 1919 had far more than merely a majority of organized labor in the capital against it.

Yours truly,
RALPH H. LUTZ

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